ABSTRACT

OF

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An Abstract of the Proceedings

OF THE

Association of Life Insurance Medical Directors of America

THIRTY-SECOND ANNUAL MEETING

The Thirty-second Annual Meeting of the Association of Life Insurance Medical Directors was held in the Board Room of the Metropolitan Life Insurance Company, No. 1 Madison Avenue, New York City, on October 20 and 21, 1921. President Augustus S. Knight was in the chair.

The following members were present at some time during the sessions:

J. L. Adams, H. B. Anderson, T. D. Archibald, A. W. Balch, W. B. Bartlett, J. T. J. Battle, W. W. Beckett, C. C. Birchard, D. N. Blakely, C. T. Brown, C. R. Burr, F. W. Chapin, L. D. Chapin, C. L. Christiernin, C. P. Clark, Henry Colt, E. A. Colton, H. W. Cook, J. N.Coolidge, T. C. Craig, R. M. Daley, E. G. Dewis, H. K. Dillard, W. W. Dinsmore, P. G. Drake, E. W. Dwight, O. M. Eakins, Z. T. Emery,

C. H. English, W. G. Exton, Paul FitzGerald, R. A. Fraser, S. W. Gadd, Homer Gage, W. S. Gardner, A. H. Gordon, Angus Graham, A. H. Griswold, F. L. Grosvenor, I. Haines, G. C. Hall, G. A. Harlow, F. H. Harnden, W. J. Hammer, E. M. Henderson, A. B. Hobbs, E. M. Holden, Ross Huston, W. G. Hutchinson, C. B. Irwin, W. A. Jaquith, A. O. Jimenis, A. E. Johann, G. E. Kanouse, M. L. King, R. J. Kissock, A. S. Knight, W. W. Knight, W. P. Lamb, J. L. Larway, J. M. Livingston, R. L. Lounsberry, L. F. MacKenzie, C. N. McCloud, T. F. McMahon, H. A. Martelle, O. F. Maxon, Paul Mazzuri, S. W. Means, J. C. Medd, William Muhlberg, J. H. North, Herbert Old, M. I. Olsen, J. A. Patton, W. A. Peterson, C. B. Piper, J. E. Pollard, W. E. Porter, J. T. Priestley, F. P. Righter, T. H. Rockwell, O. H. Rogers, R. L. Rowley, E. F. Russell, H. C. Scadding, S. B. Scholz, Jr., J. M. Smith, Morton Snow, H. B. Speer, J. B. Steele, Brandreth Symonds, Harry Toulmin, F. L. Truitt, J. P. Turner, H. G. Tuttle, C. A. VanDervoort, G. A. Van Wagenen, W. R. Ward, W. H. E. Wehner, F. S. Weisse, E. A. Wells, F. C. Wells, F. L. Wells, C. D. Wheeler, C. F. S. Whitney, T. H. Willard, C. H. Willits, M. C. Wilson, Glenn Wood.

The total attendance at all sessions was 113.

On motion the reading of the minutes of the last Annual Meeting was waived.

The Secretary read the minutes of the meetings of the Executive Council, held on May 25, 1921, and October 19, 1921. On motion by Dr. Toulmin, seconded by Dr. Willard, the minutes were approved as read.

The names of the following candidates for membership recommended by the Executive Council were presented:

Dr. John W. Amesse, Medical Director of the Capitol Life Insurance Company of Colorado, of Denver, Colorado.

Dr. Edward B. Bigelow, Assistant Medical Director of the State Mutual Life Assurance Company, of Worcester, Mass.

Dr. Cecil C. Birchard, Medical Officer of the Sun Life Assurance Company of Canada, of Montreal, Quebec, Canada.

Dr. Leverett D. Bristol, of the Penn Mutual Life Insurance Company, of Philadelphia, Pa.

Dr. William H. Browne, Medical Director of the American 'Life Insurance Company, of Detroit, Michigan.

Dr. Frank H. Carber, Medical Inspector of the Mutual Life Insurance Company of New York, of New York City, N. Y.

Dr. Edwin Grafing Dewis, Medical Supervisor of the Prudential Insurance Company of America, of Newark, N. J.

Dr. Percy G. Drake, Assistant Medical Director of the Travelers Insurance Company, of Hartford, Conn.

Dr. Frank H. Harnden, Assistant Medical Director of the Travelers Insurance Company, of Hartford, Conn.

Dr. Ernest M. Henderson, Medical Director of the Confederation Life Association, of Toronto, Ontario, Canada.

Dr. Jerome F. Honsberger, Medical Director of the Mutual Life Assurance Company of Canada, of Waterloo, Ontario, Canada.

Dr. Lefferts Hutton, Medical Inspector of the Mutual Life Insurance Company of New York, of New York City, N. Y.

Dr. William R. Miller, Associate Medical Director of the Ætna Life Insurance Company, of Hartford, Conn.

Dr. James M. Smith, Assistant Medical Director of the American Central Life Insurance Company, of Indianapolis, Indiana.

Dr. Malcolm K. Smith, Medical Inspector of the Mutual Life Insurance Company of New York, of New York City, N. Y.

Dr. Frank L. Truitt, Medical Director of the Reserve Loan Life Insurance Company, of Indianapolis, Indiana.

On motion by Dr. Weisse, seconded by Dr. Willard, the Secretary was instructed to cast a ballot in favor of the election of each of these candidates. The Secretary announced the ballot so cast and the candidates were declared elected. The President appointed Dr. C. D. Wheeler and Dr. C. F. S. Whitney a Committee to introduce the newly elected members to the Association. The following new members were present and were escorted into the room and introduced: Drs. Harnden, Dewis, Henderson, Truitt, Drake, and Birchard.

In the absence of Mr. Haley Fiske, President of the Metropolitan Life Insurance Company, due to illness, Mr. Fred Ecker, Vice-President, welcomed the Association. Mr. Ecker said:

Mr. President and Members of the Association of Life Insurance Medical Directors: It is a little embarrassing to be called into a meeting of such distinguished gentlemen. I take it for granted that Dr. Knight has already welcomed you to the hospitality of the Metropolitan Life Insurance Company, and there remains for me only on behalf of my fellow officers to endorse very heartily everything that he has said. If it were not for the fact that Mr. Fiske is under the domination of one of your profession, I am sure that it would be a great pleasure for him to welcome you in person.

Wasn't it Lincoln that said "God must have loved the common people because he made so many of them"? By the same token you may judge our regard for Medical Directors. You know we have only one President of this Company but we have two Medical Directors. We have something like nine Vice Presidents, but there are sixteen Assistant Medical Directors. Of course we have only one Actuary and four Assistant Actuaries, so there you are. Our late President, Mr. Hegeman, was always fond of saying it wasn't enough for him for a friend when he did well, to pat him on the back and tell him so—he liked to be told of his mistakes, to have his faults pointed out—"that," he said, "is the true act of friendship." Now, gentlemen, if you can just point out our mistakes,—if in the deliberations of this body it transpires that it is evident to you that there are faults in the conduct of our business to which you can call the Executives' attention, go the limit in constructive criticism. We will be glad to receive your suggestions.

I suppose it isn't enough for you, or it isn't enough to justify this convention, that the few of you who are permitted to present papers shall receive the personal gratification that comes from addressing this meeting,—it is a high privilege no doubt; I suppose it isn't enough that you have an enjoyable time. I take it for granted that the purpose is accomplishment, and that we can only wish for you that in your visit under the roof of the Metropolitan, this will be a notable convention, a notable convention for accomplishment.

And now I just want to say to you that you are very, very

welcome. If it happens that we have facilities here that you can make use of, particularly those of you who are from out of town, we want you to feel very free to use them. And please make use of these two Medical Directors and these aforesaid numerous Assistant Medical Directors, to the end that you will be taken care of while you are the guests of the Metropolitan. We are very glad indeed to welcome you.

Dr. Knight, the President of the Association, delivered the following address:

Gentlemen—We are coming together most happily now, after another year of unprecedented prosperity of life insurance companies. The anxious expressions of 1918 and 1919 have long since left the faces of Medical Directors, many of whom were expected at that time by their executives to immediately stop the ravages of influenza with its death claims that appeared to be wiping out everything in sight; and they and the doctors in general were severely blamed then by those executives for not knowing just how to prevent and to cure that disease with its dreadful complications. The year of exceedingly low mortality and of great increases of business that followed in 1920 brought us happily and with few cares to the Annual Meeting last October. But now in 1921 we have had throughout the United States and Canada a general death rate even lower, so that in this Company of ours, for instance, the rate for the first nine months of this year both in the Ordinary and Industrial Departments was about one-fifth less than for the same period last year, although 1920 was the lowest one on record.

We still have our work and our routine problems, not the least of which is to find medical examiners in the field, who are still willing to do careful, painstaking work and at fees which the companies can afford to pay out of the premiums received. But with all the disturbances of financial structures around us, we are carefree in the knowledge that these life

insurance companies by which we are employed are absolutely sound and successful, and it seems to me, therefore, that this is a good time to dig into our experiences and see what we can learn and profit by them.

I shall therefore present for your consideration two studies. The first is the Final Report on Mortality of Cases Rejected During 1905 to 1915 Because of Urinary Impairments. The work was done by Louis I. Dublin, Ph.D., Statistician of the Metropolitan Life Insurance Company, and it is to him that I am indebted for these facts.

We have completed an investigation of the mortality experienced by the applicants rejected by the Company because of the presence of either albumin, albumin and casts, or sugar during the period 1905 to 1915. The total number of separate cases was 6904 corresponding to 42,275 years of life exposed. There were 721 deaths up to and including December 31, 1918, which was considered the closing date of the investigation. The study included every renal case of the above types rejected during this period except those which gave in addition a history of renal colic, kidney disease, or where there were complications of heart disease.

The material is a composite of two groups. There are, first, those cases whose subsequent mortality experience was traced through the records of previous insurance in this and in other companies. The second group of cases consists of those whose mortality experience we have traced through the help of commercial agencies, through correspondence with our field force, and with personal physicians, whose names appeared on the application blanks. These agencies were most coöperative and their reports were apparently made after careful investigation. We believe the results to be reliable.

We shall consider our material under three main subdivisions:

1. Cases with albumin alone.
2. Cases with albumin and casts.
3. Cases with sugar.

1. Cases with albumin.

This group consists of 2073 persons, covering 12,078 years of exposure and 161 deaths. There was a microscopic examination made in all of these cases and the absence of casts was confirmed. The following table shows the mortality history of these cases according to attained age:

TABLE I

MORTALITY AMONG REJECTS SHOWING ALBUMIN, ALL QUANTITIES

Exposures, Deaths, and Death Rates per 1000 and Ratios of Actual to Expected Deaths by Various Tables

Attained Age	Exposure	No. of Deaths	Death Rate per 1000	RATIO OF ACTUAL TO EXPECTED DEATHS			
				Amer. Exp.	Intermed. Exp.	Special Class A	Special Class B
All ages	12,077.88	161	13.33	124	113	76	59
15 to 24	2,134.58	12	5.62	72	91	45	36
25 to 34	4,490.84	37	8.24	98	107	64	50
35 to 44	3,067.73	48	15.65	159	142	103	83
45 to 54	1,661.65	37	22.27	162	120	96	72
55 to 64	618.32	23	37.20	140	105	77	55
65 and over	104.76	4	38.18	63	53	33	25

Taken as a whole, this group can be considered only for Special Class insurance. As we shall see later, some of the cases will make good Special Class A business, with its mortality expectation of 150–200%, others Special Class B business, with its mortality expectation of 200–250%, and still others will require rejection, depending upon the amount of albumin found and the age of the applicant. It is significant, however, that so large a group of cases which were rejected should give a ratio of actual to expected of 76 on our Special Class A table.

We have subdivided these cases according to the amount of albumin, distinguishing:

A. a faint trace of albumin.
B. a trace of albumin.
C. a moderate or large quantity of albumin.

Of these three classes, the third was too small to give reliable results. We, therefore, present below the ratios of actual to expected mortality for the first two classes according to the several tables of mortality with distinction of age at time of examination.

TABLE II

MORTALITY AMONG REJECTS SHOWING "FAINT TRACE" AND "TRACE" OF ALBUMIN

Ratios of Actual to Expected Deaths, by Various Tables, Under and Over Age 40 at Examination

Table	Faint trace of albumin		Trace albumin	
	Under age 40 at exam. (4,789 years of life)	Age 40 and over at exam. (1,973 years of life)	Under age 40 at exam. (3,628 years of life)	Age 40 and over at exam. (799 years of life)
American Exp. Table	108	132	109	231
Intermed. Exp. Table	112	100	115	175
Special Class A	69	74	70	135
Special Class B	56	55	57	99

The first group is more favorable than the second, especially at the older ages. Cases of constant *faint trace* of albumin should be acceptable on the Special Class A Table at all ages. Cases showing a constant *trace* of albumin are acceptable only *under* 40 on the Special Class A plan. Those over 40 showing a trace or more of albumin constantly cannot be accepted, but it is suggested that in such cases, additional specimens be

obtained at once unless the sedimentary picture shows that a period of delay is necessary. Those giving smaller quantities of albumin in subsequent examinations may then be considered for Special Class insurance. The rest may be postponed or rejected as is indicated by the findings.

2. Cases with albumin and casts.

This group consists of 3264 lives, giving 22,011 years of exposure, with 452 actual deaths. The following table gives the facts of mortality according to attained age:

TABLE III

MORTALITY AMONG REJECTS SHOWING ALBUMIN AND CASTS, ALL QUANTITIES

Exposures, Deaths, and Death Rates per 1000 and Ratios of Actual to Expected Deaths by Various Tables

Attained Age	Exposure	No. of Deaths	Death Rate per 1000	RATIO OF ACTUAL TO EXPECTED DEATHS			
				Amer. Exp.	Intermed. Exp.	Special Class A	Special Class B
All ages	22,011.03	452	20.54	162	139	97	74
15 to 24	2,176.49	29	13.32	171	215	107	86
25 to 34	6,424.12	57	8.87	105	115	68	55
35 to 44	6,381.57	117	18.33	187	166	121	98
45 to 54	4,396.09	114	25.93	188	139	111	83
55 to 64	2,209.69	109	49.33	187	139	103	73
65 and over	423.07	26	61.46	102	85	53	41

The group, taken as a whole, is eligible only for Special Class B insurance, the ratio of actual to expected being 74 per cent. of the Special Class B table.

These cases were subdivided according to whether hyaline or granular casts occurred. We found that the mortality, especially under age 40, was much better for the cases with hyaline casts than for those with granular casts. The most

important difference for us to keep in mind, however, is as to the amount of albumin found. The following figures give the ratios of actual to expected according to the several mortality tables for cases with various amounts of albumin and casts, distinction being made for ages under 40 and over 40 at application.

TABLE IV

MORTALITY AMONG REJECTS SHOWING "FAINT TRACE," "TRACE," AND "MODERATE OR LARGE QUANTITIES OF ALBUMIN WITH CASTS"

Ratios of Actual to Expected Deaths, by Various Tables Under and Over Age 40 at Examination

Table	Faint Trace Albumin		Trace Albumin		Moderate or Large Quantity Albumin	
	Under age 40 at exam. (6698 years of life)	Age 40 and over at exam. (4563 years of life)	Under age 40 at exam. (6163 years of life)	Age 40 and over at exam. (2626 years of life)	Under age 40 at exam. (1541 years of life)	Age 40 and over at exam. (419 years of life)
American Exp. Table.	119	139	148	224	279	382
Int. Exp. Table......	117	105	149	169	288	284
Special Class A......	77	77	95	126	179	209
Special Class B......	62	57	77	92	145	154

This table clearly indicates that cases with a *faint trace* of albumin and casts may be considered for Special Class A insurance irrespective of age at application. Those with a *trace* of albumin and casts are eligible for Special Class B insurance only, and that at age *under* 40. Those over 40 and those having large quantities of albumin on several successive examinations should be postponed or rejected.

3. Cases with sugar.

In this group, there were 1567 persons with a total of 8186 years of exposure and 108 actual deaths. The following tables present the facts by attained age:

TABLE V

MORTALITY AMONG REJECTS SHOWING SUGAR, ALL QUANTITIES

Exposures, Deaths, and Death Rates per 1000 and Ratios of Actual to Expected Deaths by Various Tables

Attained Age	Exposure	No. of Deaths	Death Rate per 1000	RATIO OF ACTUAL TO EXPECTED DEATHS			
				Amer. Exp.	Intermed. Exp.	Special Class A	Special Class B
All ages	8,185.55	108	13.19	111	94	68	52
15 to 24	631.67	6	9.50	120	154	76	61
25 to 34	2,152.16	16	7.43	88	96	57	46
35 to 44	2,672.66	26	9.73	99	88	64	52
45 to 54	1,986.57	34	17.11	124	92	73	55
55 to 64	705.00	22	31.21	118	88	65	46
65 and over	37.49	4	106.69	222	174	111	82

The group as a whole suggests Special Class insurance but it is important to distinguish the cases as we have done above with albumin, according to the amount of sugar. When this distinction is made, the number of cases is unfortunately reduced so that no very safe conclusions can be drawn. Nevertheless, the results are suggestive and are presented herewith. The following shows the ratios of actual to expected on the several mortality tables according to the quantity of sugar.

TABLE VI

MORTALITY AMONG REJECTS ACCORDING TO AMOUNT OF SUGAR

Ratios of Actual to Expected Deaths, by Various Tables

TABLE	Faint trace sugar (2282 years of life)	Trace sugar (1291 years of life)	Moderate and large quantity sugar (4612 years of life)
American Exp. Table....	58	91	143
Int. Exp. Table.........	51	78	119
Special Class A.........	36	55	86
Special Class B.........	28	42	65

If the number of cases were sufficiently large we would be justified in making no rating on applicants with a *faint trace* of sugar provided they were otherwise in good physical condition. A ratio of 58 per cent. of the American Experience Table is as good as we can ask for from good Ordinary business. Cases with a *trace* of sugar according to our findings would be eligible for Intermediate insurance. Those with a *moderate or large amount* of sugar form a more difficult group to handle. They include cases with a large trace of sugar at one extreme and cases with very large quantities on the other. Obviously, we must reject the cases with very large amounts. Only the more favorable applicants can be accepted even though the ratio of actual to expected mortality for the group as a whole is 65 per cent. of the Special Class B table.

Of these 1567 rejects on account of sugar there were 216 who were 20% or more overweight, and the mortality ratio among them was:

American Experience Table	149	Actual deaths 23
Intermediate Experience	120	
Special Class A	90	
Special Class B	67	

Whereas among the 1351 of these cases that were of normal weight, age distribution the same, the mortality was:

American Experience	106	Actual deaths 85
Intermediate Experience	91	
Special Class A	64	
Special Class B	49	

Furthermore, it was interesting to note that the amount of sugar found on examination among the overweight rejects was larger on the average than among the normal weights, as follows:

Amount of Sugar	*Per Cent. Among Overweights*	*Per Cent. Among Normal Weights*
Faint Trace	15	27
Trace	10	16
Moderate Quantity	75	57

The actual cause of death was given as diabetes at the rate of 615.7 per 100,000 among overweights, whereas it was 283.7 per 100,000 among normal weights who had been rejected on account of sugar.

Our results with the sugar cases are much more favorable than we expected in view of the prevailing opinion on the gravity of sugar in the urine. We hesitate, therefore, to make very definite recommendations. The figures are presented for what they are worth as a suggestion. Our policy in dealing with applicants showing small quantities of sugar has undoubtedly been too severe. In the Medico-Actuarial investigation cases where sugar was found in the urine, once in several examinations, did not give an exceptionally high mortality rate. The ratio of actual to expected was only 103 per cent. of the Medico-Actuarial table, which is considerably lower than the mortality given by our group taken as a whole. Our rejections were more seriously impaired lives. The medical literature also indicates that diabetes is often of long duration. Our findings, therefore, appear to be significant although based on a limited experience. We shall continue to collect data on sugar cases and report at a later date.

GENERAL COMMENT

When albumin, albumin and casts, or sugar are found in specimens, we should not as a routine postpone for three to six months as we have generally done in the past. As already pointed out, this policy results in the loss of a large amount of business; for a little more than ten per cent. of postponed cases finally take insurance in the Company. Wherever additional specimens are required to make a judgment, we should obtain them at once when it is easy to do so. Our decision should be based on the second or third finding. We should keep in mind not only the prognosis of the individual case but, in addition, the probable mortality of the group to which the individual belongs. Our study has shown that, as a group, many of these cases can be given Special Class A or Special Class B insur-

ance. We shall certainly make mistakes in individual instances in following this formula; but, unless our study is fallacious, we can make no mistake with the group as a whole. If, for example, we were to take all individuals who showed a faint trace of albumin without casts for Special Class A insurance, we would, according to our findings, come out ahead in the long run although some of these people would die at an early date from Bright's disease. It should and has been our policy, of course, to reject cases where there is no doubt of the presence of disease. But, all cases where there is doubt should be given the benefit of that doubt and issued on the plan indicated by these experiences. The effect of such a policy, if extensively followed, should be to put on the books of the Company a large amount of desirable business, such as had been previously lost by postponements or by waiting for too many examinations.

Based upon the results of this study, the Metropolitan Life Insurance Company has for several months now been selecting these risks with the following limitations:

ALBUMIN—ACCIDENTAL

Present in one (the first) of several (not less than 3) tests. No rating.

ALBUMIN—INTERMITTENT

Present in two out of three or four tests:

Less than a trace—under age 30—Intermediate (120–150% mortality)
Less than a trace—30 or over—Special Class A (150–200% mortality)
Trace—under 40—Special Class A (150–200% mortality)
Trace—40 or over—Special Class B (200–250% mortality)
More than a trace—all ages—P. P. or Rej.

ALBUMIN—CONSTANT

Present in both of two examinations or three out of three or four examinations.

Less than a trace—all ages—Special Class A
Trace—under age 40—Special Class A
Trace—40 or over—P. P. or Rej.
Large trace or more—all ages—P. P. or Rej.

If first specimen contains a large trace or less, get another specimen rather than postpone.

I will mention here simply for comparison, that the New York Life Insurance Company rates these cases, as I understand it, as follows:

	Ages		
	Under 30	30–45	Over 45
	Constant Albuminuria		
Faint trace	+ 10	+ 20	+ 25
Trace	+ 25	+ 35	+ 50
Moderate	+ 40	+ 50	+ 75
Large amount	+100 up	+125 up	+150 up

In that Company their ratings are even more liberal than those we are now following, but as our experience accumulates we, too, may be more generous.

ALBUMIN AND CASTS

Different findings in different specimens make it impossible to cover all cases, but the following suggestions will apply:

ALBUMIN

Less than a trace—few or less casts (Hyaline or Granular); all ages—Special Class A

Trace—few or less casts (Hyaline or Granular); under age 40—Special Class B

Trace—with casts (Hyaline or Granular); age 40 or over—P. P. or Rej.

More than a trace—with casts (Hyaline or Granular); all ages—P. P. or Rej.

These ratings to be given when casts are found with albumin in both out of two, or two out of three specimens.

Two or more specimens in all cases (three generally sufficient).

If first specimen contains no more than a large trace of albumin and a "few" casts, get another specimen.

The second study for your consideration is that of:

PERIODIC HEALTH EXAMINATIONS

There is such widespread inquiry and consideration nowadays by business men, heads of institutions, physicians in charge of industrial plants, and by Medical Directors and executives of life insurance companies as to whether periodic examinations of employees or of policyholders, coupled with proper advice to those who are found to have impairments, are worth while,—whether the resulting mortality among those who are thus examined is in fact sufficiently lowered to pay for the cost of it—and these questions are so difficult of categorical answer, so increasingly difficult when the complete actuarial demonstrations are required—that I think it may be profitable to present here as fully and as fairly as I can the experiences which the Metropolitan Life Insurance Company has been having with periodic health examinations of its Ordinary policyholders by the Life Extension Institute since February, 1914. Since that time the descriptive leaflets about the privilege have been sent to every Ordinary policyholder with every premium notice. These leaflets urge the examinations, tell the policyholders that the results will not be furnished to the Company, that these are strictly confidential between them and the Institute, which is a separate corporation, and that the examinations will not affect their insurance in any way. The leaflets also tell how if they are insured for $1000 they can be examined after the policy has been in force for four years and every fourth year thereafter; if for $2000, after it has been in force three years, and every third year thereafter; if for $3000, after it has been in force for two years, and every second year thereafter, and if over $3000, after it has been in force for one year and every year thereafter. The Institute has described the examination that it gives as:

(1) A statement (made principally by the policyholder himself) of the physical condition and personal and family history of the policyholder.

(2) A thorough physical examination by the Institute's local examiner, covering an examination of the heart, lungs, abdominal organs, nervous system, vision, nose and throat, physique, blood pressure, and general bodily condition.

(3) An examination of the urine at the Home Office Laboratory of the Institute covering chemical and microscopical conditions.

Under this privilege during the seven years and seven months—February, 1914, to July 31, 1921—94,998 initial and subsequent examinations have been authorized and this is about 3.79% of the estimated maximum number of eligible requests that could have been received, which is 2,507,944. And of these that asked for those examinations and had them authorized, 62,478 actually were examined completely or partially and this is about 2.49% of the 2,507,944 that could have been examined. As to the matter of cost of the examinations, the Company has paid the Institute:

Year 1914	$11,346.40
Year 1915	10,733.05
Year 1916	10,474.35
Year 1917	25,284.60
Year 1918	38,533.85
Year 1919	43,569.95
Year 1920	44,050.55
Year 1921—First 7 months	41,114.30
Total amount paid during 7 years 7 months	$225,107.05

It can be seen at a glance that most of these examinations were of too recent date to permit of much enlightenment from a study of the subsequent mortalities among them, but our Statistician, Dr. Dublin, has made an exhaustive survey of those who were examined during the first two years of the experiment—1914 and 1915—and those examined in later years can be studied similarly when the experiences are old enough. For this purpose and for this statistical study only the original examination records were put at our disposal by

the Institute. Our study covered 5987 males whose physical examinations were complete. We also have a record of all subsequent examinations made on these persons.

Our method of study was to determine the actual mortality experienced by these 5987 male persons during the years subsequent to their first examination up to November 15, 1920, when the period was arbitrarily closed. We compared our findings, with due regard to the age of the examined, with the expected mortality according to certain standard tables.

The group as a whole had an exposure of 33,629 years, making an average of about five and one-half years per person since the initial examination. The tracing through the Company's records showed that there had been actually 217 deaths among the close to six thousand persons during this period. There should have been 412 deaths according to the American Experience Table, and 303 deaths according to the American Men Table (Ultimate). In other words, the group as a whole gave a mortality rate 53 per cent. of the American Experience Table, and 72 per cent. of the American Men Table (Ultimate).

The policyholders examined by the Life Extension Institute had a lower death rate than that expected by the various tables at practically every age period. By age groups the actual mortality compared with the expected by the American Men Ultimate Table, follows and shows a favorable mortality for all age groups except 70 and over, bearing in mind, however, that the exposures for ages under 30 were insufficient to give reliable data. It seems as though the favorable mortality for the group as a whole resulted from the very favorable experience among those aged 40 to 60, inclusive.

Attained Age	*Per Cent. Actual Claims to Probable A. M. (5)*
Under 30	83
30–39	85
40–49	78
50–59	47
60–69	86
70 and over	79

It is evident that these examined persons were highly self-selected and very favorable lives. This is confirmed by the fact that about one half of them actually had $5000 or more of insurance in the Company at the time of their examinations. They represent economically the most select lives in the Ordinary Department, in their favorable occupations and in their knowledge of and interest in personal hygiene and probably in their determination to follow such courses as will keep them well and prolong their lives. All of these points help to explain the very low death rate which occurred among them.

To determine further the value of the examinations, we must take into consideration the constituency of the group examined. We must determine the kind and the extent of the impairments found at the time of these examinations and determine the mortality of the respective classes into which the material must be divided. In this connection we had the assistance of Assistant Medical Director Coolidge, who went over the records with the Statistician and evaluated the impairments found. As the result of this work, we were able to divide our material into four main classes. The first consisted of 1620 persons, or 27 per cent. of the total, whose defects were so trivial that we did not hesitate to consider them Preferred risks. These persons would correspond to our best $5,000 Whole Life policyholders, with their mortality expectations of 100% or less. The second group consisted of 1269 persons or 21 per cent. of the total, who showed minor impairments on the examination, such as, slightly thickened arteries, some digestive disturbance, functional heart defects, etc., which would have justified issuing Ordinary insurance on their lives but would have excluded them from the $5000 Whole Life group according to our present Medical Division standards. The third group consisted of 1728 persons, or 29 per cent. of the total, who had no very serious impairments except that they showed albumin in the urine in amounts from a very slight trace to and including a large trace. It is our present practice to limit the great majority of these to substandard policies. These three classes make up more than three-fourths (77 per

cent.) of the total. The rest, a little less than one-quarter, included principally persons whose impairments would ordinarily be considered serious enough to warrant rejection for standard policies. They include persons with high blood pressure, organic heart diseases, lung troubles, and other organic diseases, although among them was a relatively small number of persons who would have been limited to Intermediate insurance with mortality expectation from 120 to 150 per cent. or to Special Class A insurance with mortality expectation from 150 to 200 per cent., or to Special Class B insurance with its expectation of 200 to 250 per cent. mortality, but their numbers are so small as not to justify any special treatment of them.

We may now consider the actual mortality as compared with the expected in each one of these four classes.

I. PREFERRED RISKS

This group, with an exposure of 9181 years of life, had 34 actual deaths. The ratio of actual to expected on the American Experience Table was 33 per cent. and on the American Men (Ultimate) Table 47 per cent. In view of the fact that 62 per cent. of these persons were still within the first five years of their insurance, we also valued them on the American Men Select and Ultimate Table and, on this basis, found their mortality 51 per cent. of the expected, which is only slightly higher than on the Ultimate Table. The group would appear at first sight to have profited from the service of the Institute in spite of the difficulties that must be met in improving the mortality of people already in excellent physical condition. We hesitate to draw this conclusion finally because it is impossible to say how much credit should be given to these persons for their interest in hygiene, as evidenced by their taking these examinations, many of them more than once. They are the kind of people who under ordinary circumstances avoid injurious excesses, take care of themselves and would, therefore, ordinarily live longer than even most super-stand-

ard policyholders, irrespective of whether they took these examinations or not.

That the examinations were found valuable and appreciated by the policyholders is indicated by the fact that 41 per cent. of them came back for subsequent examination. A number availed themselves of the privilege each year in spite of their fine physique.

2. ORDINARY RISKS

The second class was exposed a total of 7,111 years and had 44 actual deaths. The ratio of actual to expected deaths on the American Experience Table was 52 and, on the American Men Ultimate 72. In this class, 54 per cent. were within five years of their original insurance examination and were, therefore, select lives. This fact is undoubtedly an element in their favorable mortality. Few of the deaths which occurred among them were from the chronic diseases. The rate from tuberculosis was especially low, 42 per 100,000, about half that in the Ordinary Department. Bright's Disease and heart disease likewise showed very low rates. There were twelve deaths from influenza and pneumonia in this group.

These people were found on examination to have such defects as functional heart disturbances, high pulse rates, somewhat thickened arteries, relatively high or low blood pressure, and other conditions which are usually considered important enough to require medical care, although not of so serious degree as to bar them from Ordinary insurance. The fact that this group had a mortality rate better than that of the Ordinary Department over the same period is a favorable indication as to the value of these examinations. Thirty-seven per cent. of this class returned for subsequent examinations.

3. CASES WITH ALBUMIN

The third group of 1728 persons with albumin gave a total experience of nearly 9850 years of life. There were 38 deaths

among them. The ratio of actual to expected was 33 per cent. on the American Experience Table and 46 per cent. on the American Men Ultimate Table. In other words, this class had a mortality rate virtually the same as that of the Ordinary Preferred, who had no impairments worthy of record. This is one of the points of our study which is very startling but we must remember that albuminurias in otherwise standard lives uniformly give low mortalities in the initial period. It must be noted also that the finding of albumin among these people was limited to one examination. We cannot say that they had persistent albuminuria. Some of them undoubtedly had persistent albuminuria; others had intermittent albuminuria and the remainder only temporary or accidental albuminuria. But, even if we disregard the presence of albumin in these persons altogether and consider them for the other defects, we find that about one half of them had conditions which would have excluded them from the $5000 Whole Life policy, although they all would have been granted Ordinary insurance. In spite of these facts, these persons experienced a low death rate in the five subsequent years. In the light of their impairments, they have done well. The facts perhaps suggest that the mere presence of albumin on one examination must not be considered in itself as a serious impairment, at least not during the next five years after examination. There were only three deaths from Bright's disease among them during the entire period subsequent to the examination, or a rate of 30 per 100,000, which is about the same as in our Ordinary Department. There were two deaths from heart disease; eleven died of influenza and pneumonia; four died of tuberculosis, or at a rate of 41 per 100,000. The group has enjoyed a favorable mortality during the period subsequent to the Institute examination. Thirty-three per cent. of them returned for reëxaminations in later years.

4. MISCELLANEOUS CASES

The fourth is a miscellaneous group. It is composed of 1370 individuals, exposed for a total of 7485 years of life. There

were 101 deaths among them and the ratio of actual to expected mortality was 94 per cent. on the American Experience Table and 117 per cent on the American Men (Ultimate) Table. This is the only group which had a higher mortality rate than obtains in the Ordinary Department. No conclusions can be drawn from the experience as a whole, however. It is too heterogeneous. There were 77 persons among them who, according to Dr. Coolidge, would have been granted Intermediate (120–150 per cent. mortality) policies if they were applicants for insurance; 202 Special Class A policies (150–200 per cent. mortality); and 48 Special Class B policies (200–250 per cent. mortality). In addition, 133 would have been postponed. This makes a total of 460 persons who are the least impaired of the group; the rest, or 910, appeared to be so seriously impaired that they would have been rejected outright for any plan of insurance issued by the Company.

For the purpose of studying this group particularly, we singled out first a group of 683 from these who would have been rejected but who were fairly homogeneous with respect to the seriousness of their impairment. Some had abnormal blood pressure with various complications; a considerable number had organic heart affections, some had evidence of lung disease. They were seriously impaired lives but not as yet very sick people. They were, we thought, immediately below the grade which would have been acceptable for Special Class B insurance where the mortality of 200 to 250 per cent. is expected. Yet, this impaired group gave a ratio of actual to expected deaths on the American Experience Table of 66 per cent. and on the American Men (Ultimate) Table, 82 per cent. This, too, is an interesting showing in view of the impaired lives which went into the group. Obviously, these people are of the class which when informed of physical defects are particularly careful to lead hygienic lives.

In our anxiety to be as certain as possible whether these findings and interpretations were correct, or how they ought to be modified, we enlisted the services of our Actuaries who studied the problem, went over all the cards and checked the

results. Their total figures differed slightly from the Statistician's because they included a number of additional cases to those used in Dr. Dublin's study, and they found a very few more deaths that had occurred during the period but that had not been reported at the Home Office before the Statistician's studies were finished. Nevertheless the general results that they obtained were very much the same "raising the total from 53 to 57 percent. on the American experience and still indicating light mortality." And inasmuch as these policyholders under study were all examined by the Institute in 1914 or 1915 the Actuaries took our total Ordinary experience on policies issued prior to 1915 and tabulated the experience by years of duration, making the policy years date from July 1st of one year to June 30th of the next year, and compared the mortality by years with that of the Institute cases. During the year July 1, 1915, to June 30, 1916, the cases examined by the Institute showed a mortality of 39 per cent. of the American Table, while the total Ordinary policies showed a mortality of 70 per cent. During the next year the total mortality remained the same, but the mortality on the lives examined increased from 39 to 47 per cent. The mortality experience of the Ordinary policies for the five years compared with the experience on those cases examined by the Institute appears in the following table:

TABLE SHOWING PERCENTAGES OF AMERICAN TABLE EXPERIENCED IN EACH YEAR FROM JULY 1, 1915, TO JUNE 30, 1920, ON TOTAL ORDINARY POLICIES ISSUED PRIOR TO 1915 AND ON POLICIES OF THOSE EXAMINED IN 1914 AND 1915 BY THE LIFE EXTENSION INSTITUTE

YEAR	Total Ordinary Policies. Per Cent. of Expected	Examined by Life Extension Institute. Per Cent. of Expected	Per Cent. of Total for Institute
July 1, 1915 to June 30, 1916	70	39	56
July 1, 1916 to June 30, 1917	70	47	67

Year	Total Ordinary Policies. Per Cent. of Expected	Examined by Life Extension Institute. Per Cent. of Expected	Per Cent. of Total for Institute
July 1, 1917 to June 30, 1918	73	62	85
July 1, 1918 to June 30, 1919	112	87	78
July 1, 1919 to June 30, 1920	66	60	91

The increasing scale indicates that had the experience been taken for the second five years instead of the first five years, the mortality ratio would not have been so low. It is the low mortality in the early years that makes the low ratios for the entire period. This point is especially important with reference to the large number of cases of albumin that were found among the examined. Experience has shown that those who show the presence of albumin have a relatively low mortality during the first five years after examination. For example: In the Medico-Actuarial investigation, it was shown that albumin cases had a mortality of 78 per cent. for the first five years and 200 per cent. for the next five years. This result for the first five years tallies also with the Metropolitan's experiences as reported by Dr. Ogden. Those who had albumin had a particularly low or favorable mortality for this initial period. What the future will show among these people is another question, although we already have some information that the rate of mortality increases with duration of policy. It would, therefore, be safe to say that the benefits of the Life Extension examinations are pretty well limited to the years immediately following the examinations, and run out at the end of about five years.

Now then, with all these facts at hand, let us try to see, if possible, what money saving, if any, there has been for the

Metropolitan Life Insurance Company from these Life Extension Institute examinations. With this in mind, we compiled an experience on our Ordinary policies using the same proportion of business by years of issue and the same proportion of $5000 Whole Life policies as that prevailing in the Institute cases. This control experience was observed from the years 1915 to 1920. We thus obtained the annual mortality on a group very similar to those examined by the Institute except as to the average amount of insurance per policy (being about three times that of the average policy in the Ordinary Department) and, also, that the control group were not medically reëxamined lives. The difference between the death rates of the two sets of figures is the first indication of the saving for each one of the five years of the experience. To obtain the ultimate saving, select death rates for the first five years were computed assuming American Men Ultimate mortality thereafter. To calculate the mortality gain, select commutation columns on a three and one half per cent. interest basis were constructed. The present value of the total mortality gains as of July, 1920, was computed as $126,477. From this amount must be subtracted the cost of the original and the subsequent examinations made on the 6000 persons examined by the Institute. This was approximately $40,000. The net gain was $86,000, and if we deduct Home Office expenses of $8900 it still leaves a profit of $77,100.

It would appear, therefore, that the Company has on this particular group of people had its principal returned and made a 200 per cent. profit on this investment, during a period of approximately five years, provided that the subsequent experiences on these lives does not exceed the expected according to the American Men Table.

I must not be interpreted as saying that any one arranging for a series of examinations, such as the Institute gives, say, for a group of employees in a factory, will save $2 over a return of $1 expended in the period of the first five years. Much will depend upon the character of the examinations and also upon the group that is selected for the examinations, and to a far

greater extent whether the examinations are optional or not. In our case, the group selected itself. In a factory or other establishment, it might be arranged to have every one take the examinations. Personally, I would not expect a similarly favorable result to follow, although the after-results might very well justify the expenditures incurred in making such examinations. I should be seriously disappointed if they did not. The employer might gain enough from the added efficiency of his employees to pay the entire expense, quite apart from the lessened mortality and morbidity. But our results must not be taken without reservations as an indication of what would follow if a whole group of persons were examined, irrespective of their own wishes. I emphasize this point, especially because I am much impressed with the importance of the factor of self-selection. The Metropolitan policyholders sought out these opportunities, and came for these examinations year in and year out, even though many of them showed no serious impairment. They were persons who take their personal condition seriously and who do the usual things to protect themselves from ill-health. They are a special lot of people, quite unlike the usual run of policyholders or employees, generally. It is this fact which colors our results, and I confess my inability to measure the effect of this fact. I do not believe, however, that it is sufficiently large to negative the highly favorable results that appeared in our experience. Certainly the favorable results for the impaired lives suggest that excellent results followed and we cannot, with fairness, omit to say that this particular organization that has been making the examinations for our Company had only begun its work in 1914, so that with an incomplete equipment and with nowhere near as good examiners as compared with those it has to-day we have every reason to believe that the examinations and the advices that our policyholders get to-day are far superior to what they were given in 1914 and 1915. We therefore think that our experiences with the Institute have been more than justified, and that the policyholders have been correspondingly benefitted thereby.

Dr. Knight—It is with extreme regret that I announce the death of two of our Canadian friends during the past year, Dr. Joseph H. Webb, of the Mutual Life Assurance Company of Canada, and Dr. Arthur Jukes Johnson, of the Confederation Life Association. Dr. J. M. Livingston has prepared a memorial of Dr. Joseph H. Webb, which the Secretary will read.

The Secretary then read the following memorial of Dr. Webb:

Dr. Joseph H. Webb

1846–1921

The late Dr. Joseph H. Webb had been a member of this Association since its first regular meeting, and he has been present at nearly every meeting since. You must all have found him a colleague who took a profound interest in the work of the organization, and I know it was with deep regret that you received the news of his death.

Dr. Webb was born near the village of Aurora, in York County, Ontario, in the year 1846, and spent his whole life in practice in his native Province. For a time Dr. Webb worked on the ancestral farm, but, having given agriculture a fair trial, decided that he would prefer an occupation in which there was more opportunity for development of his naturally studious mind. He thereupon took up the profession of teaching, which was followed for three years. But neither did this experience satisfy Dr. Webb's ambitions, and after a short trial of pedagogy he took up the study of medicine, his life work. He pursued his studies in the city of Toronto under the late Honorable John Rolph, M.D., LL.D., and was graduated in the year 1869 with honors. For some time after his graduation Dr. Webb practiced in the town of Ayr, but afterward removed to Waterloo, where he spent the remainder of his long

life. In 1872 he married Helen Buchanan, who, together with a daughter, Mrs. T. H. Hall, of Toronto, survive him.

Dr. Webb had a large and lucrative private practice, and in addition to this was retained by The Mutual Life Assurance Company of Canada as Medical Director. He had been appointed Associate in the year 1881 and four years later succeeded Dr. J. W. Walden as the sole Medical Director. At the time of his death, in January of the present year (1921), Dr. Webb had been associated with the Mutual Life for nearly forty years. Throughout all his life he was a devoted student —as a youth at the Newmarket Grammar School, as a student of pedagogy at the Toronto Normal, and, later, as a medical student. Dr. Webb was an enthusiastic worker. During the long years through which he was privileged to practice his profession he still kept up-to-date in his reading in spite of the strain incidental to his private practice and his Company duties.

In addition to his professional work Dr. Webb engaged in many social activities. Especially during the late war did he distinguish himself by his recruiting efforts and various services connected with different departments of war work. Indeed Dr. Webb took the struggle greatly to heart, and his friends marveled at what he was able to accomplish at such an advanced age. There was no limit to his patriotism. Dr. Webb had not only his heavy practice and his strong social and political interests, but he was also a devoted servant of the Church, being a prominent officer and supporter of St. Paul's Presbyterian Church, Waterloo.

He was a man of strong convictions, but always courteous, and one need but see his countenance and carriage to realize that he was a gentleman of sterling qualities and a true friend. It might be said of him as of an English statesman in one of the noblest epitaphs ever written—"He lived usefully."

It was moved by Dr. Willard and seconded by Dr. Toulmin that this memorial be spread upon the minutes of the Association. The motion was carried.

Dr. Knight—Dr. Scadding has been good enough

to prepare a memorial to be presented to this meeting of Dr. Arthur Jukes Johnson, which the Secretary will read.

The Secretary read the following memorial of Dr. Arthur Jukes Johnson:

Dr. Arthur J. Johnson

Dr. Arthur Jukes Johnson was appointed Medical Director of the Confederation Life Association in March, 1909, and rendered faithful and valued service to the Association during that period which was terminated by his death on June 9, 1921.

He was born in Toronto and was the son of the Rev. W. A. Johnson, Rector of Weston, and founder of Trinity College School. He received his preliminary education at the Model Grammar School, Toronto, and Trinity College School. He graduated B.A. from the Trinity University and M.B., Toronto University, in 1870. After graduation he studied in London where he was on the House Staff of St. Thomas' Hospital. He then became a member of the Royal College of Surgeons of England, and on his return to Toronto, practiced there until the time of his death.

For some years he was lecturer in Anatomy and Pathology at Trinity Medical College, but it was especially as an expert in medical jurisprudence that he was most widely known. In 1902 he was appointed Chief Coroner for Toronto, and held that post until his death. He was an adviser and expert witness for the Crown in a number of celebrated criminal cases and was the author of "Inquests and Investigations—Procedure for Coroners," 1911. He was one of the most widely known members of the medical profession in Canada and had a very large circle of close personal friends. He was a gentleman of the Old School, possessing a charming manner and a geniality of disposition not often met with; was one of the most friendly and approachable men, kind hearted, sympathetic, and a most entertaining conversationalist. He was one of the best after-dinner speakers in Toronto and at the numerous

medical and other dinners he attended, his remarks were always one of the chief features of the evening. His long and varied experience of life had given him a remarkably broad outlook.

His death removes one of the outstanding figures in the medical profession in Canada and a valued member of this Association.

It was moved by Dr. Symonds and seconded by Dr. Willard that this memorial be spread upon the minutes of the Association. The motion was carried.

Dr. T. H. Willard, Chairman of the Nominating Committee, presented the following report:

New York, Oct. 18, 1921.

The Nominating Committee desires to submit the following report:

At a meeting held this day, all of the members being present, the following nominations were unanimously adopted and are herewith submitted:

For *President*	Dr. THOMAS F. MCMAHON, Manufacturers Life Insurance Company, of Toronto, Ont., Canada.
" *First Vice-President*	Dr. FRANK L. GROSVENOR, Travelers Insurance Company, Hartford, Conn.
" *Second Vice-President*	Dr. WILLIAM R. WARD, Mutual Benefit Life Insurance Company, of Newark, N. J.
" *Secretary*	Dr. ANGIER B. HOBBS, New York Life Insurance Company, of New York City, N. Y.

For *Treasurer* — Dr. CHARLES L. CHRISTIERNIN, Metropolitan Life Insurance Company, of New York City, N. Y.

" *Editor of the Proceedings* — Dr. ROBERT M. DALEY, Equitable Life Assurance Society of New York City, N. Y.

" *Members of the Executive Council* — Dr. GEORGE A. VAN WAGENEN, Mutual Benefit Life Insurance Company, of Newark, N. J.

Dr. EDWIN W. DWIGHT, New England Mutual Life Insurance Company, of Boston, Mass.

Dr. J. ALLEN PATTON, Prudential Insurance Company, of Newark, N. J.

All of which is respectfully submitted.

W. EVELYN PORTER, M.D.
CHESTER T. BROWN, M.D.
C. F. S. WHITNEY, M.D.
ROBERT M. DALEY, M.D.
WILLIAM R. WARD, M.D.
THOMAS H. WILLARD, M.D.—*Chairman.*

Dr. Knight—You have heard the report of the Nominating Committee. Are there any further nominations?

No further nominations being presented, it was moved by Dr. Jaquith and seconded by Dr. Toulmin that the nominations be declared closed and the Secretary instructed to cast a ballot on the morning following in favor of the election of the Officers and

Members of the Executive Council so nominated. The motion was carried.

The Treasurer read his report. The Auditing Committee appointed by the Chair, Dr. Weisse and Dr. Rockwell, reported that they had audited the report and found it correct, and the report of the Treasurer was accepted and placed on file.

Dr. Rogers read the report of the Special Committee in charge of the M. I. B. On motion this report was accepted and placed on file.

Dr. Thomas H. Willard, Chairman of the Committee on Public Health, presented the following report:

REPORT OF THE COMMITTEE ON PUBLIC HEALTH

At our last meeting the following Committee to be known as the Public Health Committee of the Association of Life Insurance Medical Directors was appointed:

Dr. F. C. Wells, Equitable
" F. L. Grosvenor, Travelers
" Brandreth Symonds, Mutual Life
" O. H. Rogers, New York Life
" T. H. Willard, Metropolitan Life, *Chairman*

One of its first duties was to act in answer to a telegram which was received at the office of the Mutual Life ten days prior to the November elections. The telegram was from Edgar B. Piper, editor of the *Portland Oregonian*, telling of the anti-vaccination amendment to be submitted to the voters at the forthcoming election and asking for an opinion as to the effect of this measure upon public health if adopted and its bearing on life insurance, if any. The time was short but several members of the Committee were able to confer and as a result the following telegram was sent:

"Mr. Edgar B. Piper, editor, *Oregonian*, Portland, Oregon. We very earnestly oppose the proposed constitutional amendment referred to in your telegram and urgently advise voters of Oregon to register an overwhelming negative at the polls. In view of the universal experience of the human race and the virtual stamping out by vaccination of a pestilence which formerly at frequently repeated intervals decimated the population, failure to continue the protection of the public health which vaccination affords would be criminal. In every instance in which vaccination has been abandoned outbreaks have occurred usually promptly. Millions are living to-day who would not be alive but for vaccination and to discard the use of this simple, safe, and sure preventive would be foolish in the extreme. The statistics of life insurance are abundant and extensive and all support the contention of the best medical scientific opinion of the world. Committee on Public Health of the Association of Life Insurance Medical Directors."

Meanwhile the campaign against this amendment and a similar one in California were being conducted by the Health Protective Association of California and the Metropolitan, and two of the Home Office representatives were present in the field addressing public meetings and coöperating with the agencies in the two States opposed to the legislation referred to. In Oregon the amendment was as follows:

"No form of vaccination, inoculation, or other medication shall be made a condition in this State for admission to, or attendance in any public school, college, university or other educational institution; or for the employment of any person in any capacity, or for the exercise of any right in the performance of any duty or the enjoyment of any privilege."

In California:

"No form of vaccination, inoculation, or other medication shall hereafter be made a condition for admission to, or attendance in any public school or other educational institution, or for the employment of any person in any public office."

These proposals were supported by organized forces who spared no expense in a publicity campaign to create a sentiment favorable to their passage. They were defeated in Ore-

gon by a vote of more than two to one, and in California by a majority of approximately 100,000.

It was believed that even though this legislation was negatived at the polls it would be necessary to continue the watchfulness of those interested in preserving public health and the Presidents' Association voted to procure as promptly as introduced, bills relating to public health matters and to furnish copies of these bills to the Association of Life Insurance Medical Directors. This was done and all the legislation introduced into the various states has been scrutinized and such proposed bills as have had any bearing on the subject have been immediately called to the attention of the various organizations devoted to the protection of the public health and among them the Welfare Department of the Metropolitan Life.

There has recently come into being the National Health Council composed of representatives of various private associations representing the American Public Health Association, the American Red Cross, the American Social Hygiene Association, Conference of State and Provincial Health Authorities of North America, Council on Health and Public Instruction of the American Medical Association, National Child Health Council, National Committee for Mental Hygiene, National Organization for Public Health Nursing, National Tuberculosis Association.

One of the primary functions of this National Health Council is an Information Bureau for its members and through the Metropolitan representative on the National Health Council, who also represents the American Public Health Association, prompt information has been given to all of the component parts of the Council of legislation inimical to public health as soon as it is introduced. Your Committee therefore has confined its activities to the work of acquainting the National Health Council with the introduction of dangerous legislation and stands ready to coöperate with each and every one of the component parts of that Council. The time can readily come when the agency and medical examiner force of all of the companies represented in the Association of Life Insurance

Medical Directors may be needed to help in very important work; that is to oppose legislation deemed to be inimical to the public health or to favor measures which are considered beneficial. It is of course understood that the question of supporting or opposing bills of this sort is an executive problem which should be handled through the Association of Life Insurance Presidents as is done with other legislation.

The Committee also wishes to draw the attention of the members to the fact that the only States which are not yet in the registration area are Arizona, Nevada, and South Dakota. An earnest effort will be made to have the legislatures of these States adopt the model registration law, governing the registration of deaths and births. The next regular session in all three will take place in January, 1923. This gives ample time and the Committee requests all the members whose Companies are doing business in those States to facilitate the passage of this law.

The Committee therefore begs leave to report this much of progress and to assert its willingness to earnestly and actively discharge the duties which have been imposed upon it by this Association.

Dr. Beckett—I should just like to say in supplementing this report that there has been a very vigorous campaign against all health measures on the Pacific Coast. As I stated at our last meeting, we have now a very well organized Association, taking in at least seventy-five per cent. of the doctors in California, together with a number of very prominent laymen, organized into the League for the Conservation of Health. Last spring at our Legislature we succeeded in knocking out all of those vicious measures and in an election recently in the city of Los Angeles, one of our candidates for Mayor had, as his slogan, Health Freedom. At the time of the nomination I think the bets would have been ten to one in his favor, but we succeeded in defeating him by so great a majority that I understand he is coming to New York to live! We are also getting rid of that bunch of people that have been so persistently try-

ing to put upon our statute books vicious health measures. I am glad to make this report. A similar League for the Conservation of Health has been organized in Oregon, and another in Washington. I met the Chairman of each of these Boards as I came through on this trip, and I am very glad to report that they are getting well organized, and we feel sure that we on the coast will not in the future have the stigma of putting these laws on our books. The profession, I believe, is better organized on the coast to-day than probably in any other part of our country. The doctors are no longer afraid to get into politics—they are getting into politics and they are succeeding along the lines I have just mentioned.

On motion duly seconded, the report of the Public Health Committee was accepted and placed on file and the Committee continued.

Dr. H. Wireman Cook presented the report of the Committee on Blood Pressure, as follows:

REPORT OF BLOOD PRESSURE COMMITTEE

By H. W. Cook, M.D.

At the request of Dr. Fisher, Chairman of the Blood Pressure Committee, I undertook to obtain an expression of opinion from a number of leading internists and teachers of medicine in regard to the best routine for taking both systolic and diastolic readings in order that a recommendation might be presented to this body in regard to a uniform and authoritative method to be given to the examiners of the different companies. The replies received show that not only are insurance companies instructing their examiners differently but that teachers of medicine are not in absolute agreement as to the best methods for taking blood pressure, though there is a sufficient majority on every point to make an impressive showing, and in our opinion to justify a strong recommendation. The wide variation in blood pressure methods obtaining

in our first grade medical schools suggested that inquiry be made as to whether the same unfortunate differences existed in regard to other chemical and laboratory methods such as urinalysis, blood examinations, etc. None of the clinicians answering apparently know of any method in use among medical schools to standardize their teaching of these procedures, so that it appears that different instructors in the same school and in different schools are teaching varying and often contradictory methods which may in part at least explain the deplorable confusion of both examiners and practitioners in regard to many simple and important clinical and laboratory procedures. In view of this condition and because the accurate training of medical practitioners is so vital to life insurance medical selection, it has appeared proper to your committee to recommend that the President of this Association address to the Association of Medical Colleges a communication reciting the confusion arising from this failure to standardize such clinical and laboratory procedures and requesting their comments on the subject.

Replies were received from the following eleven clinicians:

Henry A. Christian	Boston	Harvard
George Dock	St. Louis	
J. B. Herrick	Chicago	
A. M. Hewlett	San Francisco	
C. F. Hoover	Cleveland	
W. T. Longcope	New York	
L. G. Rowntree	Rochester, Minn.	Mayo
James W. Means	Boston	
G. Canby Robinson	Baltimore	Johns Hopkins
W. L. Thayer	"	" "
L. F. Barker	"	" "

This subject is of such importance and interest to us that I feel you will be interested in each of the letters.[1] Analyzed, the letters show that of the eleven men, nine are strongly of the

[1] Letters attached.

opinion that the change in sound at end of third phase should be taken as representing the diastolic or minimum pressure. Herrick alone recommends the disappearance of sounds as being most practical, and Christian says it makes no difference which point is taken.

Nine of the eleven believe that the systolic should always be taken by auscultation, the preference being at the point, as the pressure falls, when the sounds first appear. Five of these nine believe that the auscultatory systolic should always be echcked by palpation. Longcope in addition would check auscultatory systolic by palpation in cases of hypertension and when sounds are faint, and Rowntree "occasionally when necessary."

Barker alone prefers systolic by palpation and says checking palpation by auscultatory is "optional and does no harm."

From the work of physiologists, *i.e.* Erlanger, and from the opinion of the leading internists, it would therefore appear that not only does the end of the third phase represent the true diastolic pressure, but that this is the point which is being taught today by the majority of the better teachers of medicine; and further, that the end of the fourth phase may be very much lower than the true diastolic pressure and therefore very misleading, not only in the individual case, but in the result of an analysis of examinations in which either phase may have been indiscriminately used, or as is sometimes given, an arbitrary average of the two. To quote from Longcope's letter: "It is important to recognize that the third phase of the changes in sounds and the disappearance of the loud sharp sounds represent the diastolic pressure, for frequently in patients who have temporary or permanent vasomotor disturbances a sound may be heard long after the cessation of the third phase. Under these circumstances one would incorrectly conclude that the diastolic pressure was abnormally low."

This confusion is particularly unfortunate as the strong trend of opinion today is to the effect that the diastolic pressure is probably of more absolute value than the systolic in throwing light upon probable longevity and the vasomotor mechanism.

To quote Longcope again: "I have found many practitioners omit the diastolic pressure altogether and draw their conclusions from the systolic pressure. This I think is unsound and all practitioners should be encouraged to estimate the diastolic." We will, therefore, assume as established that a knowledge of the diastolic pressure is desirable and that the end of the third phase represents the correct diastolic pressure. What then is the argument in favor of recommending the disappearance of all sounds to insurance examiners? Obviously there is only one, namely, that the average examiner is not capable of recognizing the correct phase and that any fool can tell the point at which all sound ceases. But is this premise correct? Robinson in his letter says: "I think the third phase is as a rule easier to read than the disappearance of sounds." And Dock says "I cannot understand why an insurance company should recommend the fourth phase, as the difficulty of getting the pressure at the more usual time is not very great." Certainly, when the medical schools are teaching the third phase, and when the graduates of the past ten years and the coming ten years are taught to read the third phase, there should be a very marked difference in definiteness between the third and fourth phase to justify the substitution of the incorrect for the correct. It is true that an ignorant careless examiner will probably not give a correct diastolic reading at the third phase, but will he be more likely to catch and report the diminishing sound at its exact point of disappearance? Such a man does not find rales, heart murmurs, or albumin, nor does he give correct systolic pressures. Should we wrongly instruct all of our examiners in order that we may obtain a correct reading from the minority of our poorest men, of a point which does *not* represent the pressure we desire? The same argument was used against asking for systolic pressure fifteen years ago. Some companies took the position that as so many examiners were unable to take blood pressures, the data obtained was valueless. Today the third phase is easier to determine for a majority of our examiners than to make a competent chest or heart or urine examination. Rather than taking too much

recognition of the more poorly trained men should we not set correct standards for all our examiners and bring the weaker ones as soon as possible to approximate these standards? Certainly in analyzing our reports and making percentage ratings it is easier to recognize and allow for the incompetency of the minority, than to correct from all the misleading data furnished from incorrect standards.

We, therefore, make the following recommendations:

1st. That the Medical Directors' Association recognize the last loud tone at end of the third phase as representing the correct diastolic pressure and so instruct their examiners, and that those companies which have trained their examiners to report the end of the fourth phase, or the cessation of all sound, or which feel that this phase gives a valuable check on the examiners, should request both phases.

2d. That in those blanks in which both phases are not requested, it be specifically stated in small type under the line for indicating diastolic pressure that the figure given represents the last loud sound at end of third phase.

It is further recommended that all companies instruct their examiners to take the blood pressure in a sitting position at the beginning of the physical examination.

It is further recommended that attention of examiners be called to the occasional difference between the auscultatory and palpation systolic and suggest that they check the auscultatory reading by palpation and report both when a difference of more than 10 mm. Hg. is discovered.

	DIASTOLIC		SYSTOLIC		SYSTOLIC	
	End of 3d Phase	*Disappearance of Sound*	*Appear*	*Disappear*	*Palpatory*	*Auscultatory*
Thayer	x	o	?	?	x	x
Robinson	x	o	x	o	o	x
Means	x	Believes it important to standardize conditions				
Rowntree	x	o	x	o	Rarely	x
Longcope	x	o	x	o	Hypertension & faint sounds	
Hoover	x	o	Palpation of femoral		x	x
Hewlett	x	x	?	?	x	x
Herrick	x	x			x	x
Dock	x	Most practical o	Lying down		x	x
Christian	x	Makes no difference	?	?	o	x
Barker		? o			x	o Optional. Does no harm.

W. T. LONGCOPE, M.D.
800 Park Avenue
New York

August 4, 1921.

DR. HENRY WIREMAN COOK,
Northwestern National Life Insurance Company,
Minneapolis, Minn.

MY DEAR DR. COOK:

In my experience, I have found that many practitioners omit the diastolic pressure altogether and draw their conclusions from an estimation of the systolic pressure. This, I think, is unsound and all practitioners should be encouraged to estimate the diastolic as well as the systolic. As a routine, the auscultatory method of ascertaining the systolic pressure has been found with us the most satisfactory, and the point at which the first sound could be heard has been adopted as that indicating the systolic pressure. In cases of hypertension, and in a small proportion of instances in which the arterial sounds are faint, the auscultatory method should be controlled by the palpatory method.

With the determination of the diastolic pressure, it is essential to use the auscultatory method, and it is important to recognize that the third phase of the change in sounds and the disappearance of the loud sharp sounds, represent the diastolic pressure, for sometimes in children, and quite frequently in patients who have temporary or permanent vasomotor disturbances, a sound may be heard long after the termination of the cessation of the third phase. Under these circumstances, one would incorrectly conclude that the diastolic pressure was abnormally low, if the disappearance of all sounds was accepted as a criterion for the estimation of the diastolic pressure.

I do not know whether any effort has been made by medical schools to bring about uniformity in instruction upon these points, nor whether the examinations of urine and blood have been standardized so that one method of examination might be adopted by different schools. An even more confusing matter

is the recording of the chemical examinations of the blood, and I believe there has been some effort made to adopt a uniform method of reporting the amounts of urea, sugar, etc., which have been determined in these examinations.

If I can be of further assistance to you, I will gladly do what I can.

Very truly yours,
(Signed) WARFIELD T. LONGCOPE.

THE NICHEWAUG
Petersham, Massachusetts
GEORGE Q. PATTEE

August 4, 1921.

DEAR DR. COOK:

Your letter in re blood pressure reached me here. I know of no effort by medical schools to establish uniform methods for blood pressure, urinalysis, etc., other than following methods described in the better texts. I feel it makes very little difference what point is taken for the readings though it is well to use the same point constantly. The differences between different methods are relatively slight, and slight variations make very little difference after all. I use the auscultatory methods at the P. B. B. H. and I think it is preferable to the palpatory. I cannot say with certainty just what end point the men are using for the diastolic reading. In high pressure cases there is often discrepancy between auscultatory and palpatory method but which is closest to the real pressure I do not know. I have never paid any attention to slight changes in blood pressure, *i.e.* say 5 mm. of Hg. up or down or even greater and I do not think there is much use in getting excited about these minor changes from slight variations in method. I can't see that it makes a tinker's damn to insurance companies whether they choose the first change in sound or its disappearance for the diastolic; the first sound with falling pressure for the systolic or some other variation. More information probably would be obtained from taking readings by any of these methods on successive days than by any single

experiment for one reading. After all "blood pressure" is too much a fetish anyhow. So long as I know whether it is high or within the usual range for normal or low most of the time, I do not pay much attention to it anyhow. If you collate the data you get on these questions, I would like to see the result to find out how much the better clinicians are bothering now about slight changes in blood pressure and what they think.

Very truly,

HENRY A. CHRISTIAN.

WASHINGTON UNIVERSITY
Saint Louis

School of Medicine
Department of Internal Medicine
Barnes Hospital,
600 South Kingshighway

September 28, 1921.

DR. HENRY WIREMAN COOK,
Northwestern National Life Insurance Company,
Minneapolis, Minnesota.

DEAR DOCTOR COOK:

Yours of August 2d just read. I cannot understand why an insurance company should recommend the fourth phase, as the difficulty of getting the pressure at the more usual time is certainly not very great. I always take the palpation pressure, but find a great many good men do not. In most cases the difference is immaterial. I do not know of any proof that the palpatory is ever more accurate than the auscultatory method. I use palpation only as a matter of interest for myself.

I do not know of any effort on the part of medical schools to bring about uniformity of instruction and hope the time will never come when we are "Chinafied" to that extent. I should think that with a little effort insurance examiners would become just as uniform in their results as others. I see the work of a great many consultants and a number of others constantly and find that as a rule there is very little difference in their

findings. Perhaps the supposed lack of uniformity in insurance work is due more to the wrong ideal. I find some who look on the standards of blood pressure in different ages and sexes too narrowly. The normal range is rather wide and the difference in most cases between the third and fourth phase is difficult to make out, or even impossible, and the fourth phase must be taken. In further work in the same patient no harm will follow from a reading variation of five or ten points.

I would advise the practitioner in taking blood pressure to use the same kind of cuff always. I prefer a semi-rigid outside, as of leather or canvas. The instrument should be capable of comparing from time to time with a standard manometer. The pressure should be taken as much as possible with the patient in recumbent position, but in some cases taking it in a sitting position does not introduce a serious error.

I prefer to put on the pressure while feeling the artery and noting the behavior of the pulse as well as the mercury or needle as the pressure is increased. Let the pressure fall to zero, then apply the stethoscope; repeat the filling running up well above the supposed systolic and let the pressure go down gradually, noting the change of sound as well as of auscultation or mercury or needle. I always make at least three auscultatory readings, being careful to let out all of the air after each one.

I hope this will be of some assistance.

Very truly yours,

(Signed) GEORGE DOCK.

Northwestern National Life Insurance Co.,
Minneapolis, Minn.

August 1, 1921.

Dr. J. B. HERRICK,
122 So. Michigan Avenue,
Chicago, Illinois.

MY DEAR DR. HERRICK:

As a member of the Blood Pressure Committee of the Medical Directors' Association, I am endeavoring to obtain ex-

pressions of opinion from several of the leading clinicians and teachers of medicine on the technique of determining blood pressure readings with the view to formulating a clear, concise, and practical method to recommend to the life insurance medical examiners of the country.

We have found at the present time that there is little uniformity between the different medical schools and also between the different insurance companies in instructions to examiners on this point, and it has been the experience of most companies to find examiners considerably confused, particularly, of course, in regard to the correct method of determining the diastolic pressure. Most medical schools recommend the third phase of the change in sound, while most insurance companies recommend the point of disappearance of all sounds, as they have felt that it is easier to train examiners to take this point accurately than the more vague third phase. A few of the insurance companies have felt that it was worth while to have the examiner take the correct diastolic reading rather than the easier and incorrect disappearance of all sounds.

Best from practical point of view.

Dr. Janeway used to insist very positively upon checking up the auscultatory method by palpation, as he felt, and I think others have had the same experience, that probably in some cases of high blood pressure the auscultatory systolic was not as correct as that indicated by palpation.

I agree with Dr. Janeway.

Do you know whether any effort has been made by the medical schools to bring about uniformity in instruction on this point, and perhaps also on such other points as, for example, urinalysis, blood examinations, etc.?

No effort that I know of.

In insurance work we have found a decided lack of uniformity, so that the results of the work of practitioners from different schools do not permit of comparative analysis.

Pardon the great brevity.

J. B. H.

8/5/21.

Would you be good enough to outline briefly the method which you feel should be recommended to the average general practitioner in taking the blood pressure, both systolic and diastolic?

Thanking you in advance for your assistance, I am,

Cordially yours,

HENRY WIREMAN COOK, M.D.,
Vice-Pres. & Medical Director.

JAMES H. MEANS, M.D.
15 Chestnut St.,
Boston.

September 1, 1921.

DR. HENRY WIREMAN COOK,
Northwestern National Life Insurance Company,
Minneapolis, Minn.

DEAR DR. COOK:

Pardon my delay in replying to your letter of July 30 on the subject of blood pressure readings. I do not believe that I can give you an opinion that will be particularly helpful to you. I do not think that any particular effort has been made to standardize the technic of taking the blood pressure at the Harvard Medical School; at least, if it has, it has not come to my attention. Personally I have usually used the third phase of the change in sound for the diastolic pressure rather than the disappearance of all sounds, and I have no very definite opinion as to which would be the best for a standard method.

I personally think that the method in itself is not nearly as important as to have the conditions under which blood pressures are taken standardized. I think the blood pressure readings should be taken with the patient always in the same bodily position, preferably lying down and after a considerable period of rest. I think that we should have our conditions of taking blood pressure reading standardized just as we do

those of making determinations of the basal metabolism. It seems to me there is more likely to be variation due to different conditions while the reading is being taken than to differences due to the actual method of taking the reading itself.

In general, however, I feel that you can give me more information than I can you and I shall be distinctly interested to hear what conclusions you finally reach.

Very sincerely yours,

J. H. MEANS.

MAYO CLINIC
Rochester, Minnesota

July twenty-eighth,
Nineteen Twenty-one.

DR. HENRY WIREMAN COOK
V.-Pres. Northwestern National Life Insurance Co.
Minneapolis, Minnesota.

DEAR HENRY:

It certainly was good to hear from you and I hope to see you some time in the very near future.

I can summarize my feelings in regard to the determination of blood pressure in a very few words. I am dependent upon the auscultatory method in the vast majority of cases. Generally speaking, I prefer a mercury apparatus, although I do use the dials very frequently. Here in Rochester an effort is make to keep them standardized. But, for our work in the wards, I have insisted upon having the mercury apparatus.

I use the palpation method only in cases that are unusual from one point or another. In extremely high or extremely low pressure it is desirable, and also in the presence of marked irregularity. Faith in the auscultatory method is based on rather large experience during past years where I have con-

trolled the auscultatory method with palpation. It is because of the close agreement found by experience that I have given up palpation as a routine. For the systolic pressure I accept the first definite sounds which come through; for the diastolic I accept the third phase, *i.e.* the sudden change in the quality of the sound, which occurs usually within a few millimeters at the level of which the sound disappears entirely.

There are two or three pieces of work with which you may be familiar already. Nevertheless, it will do no harm to mention them. Hooker checked the Erlanger apparatus with the auscultatory method by taking phonographic tracings at the same time and showed graphically that the third phase corresponds to the diastolic pressure as indicated by the Erlanger apparatus. Erlanger has been carrying out some very interesting work in relation to the Karatkoff's sounds. He has been able to reproduce these sounds outside of the body along the course of long rubber tubes partially filled with water. He sends an impulse down the tube, locates the areas at which these sounds appear, and shows that they disappear in the intervening sections. Just recently he has been making photographic studies of the change in the vessel itself during the production of these sounds. He reported this work before the American Physicians Society in Boston last summer.

If you are giving a paper on the subject of auscultatory methods I should very strongly advise you to get hold of Erlanger's work, as it would be of tremendous interest to you and also to any audience. If the work is not published I should be willing to write him asking him to send you a carbon copy of his paper.

I am enclosing a list of physicians in Boston, New York, Baltimore, Chicago, St. Louis, San Francisco and Cleveland.

With very best regards and all good wishes to your family, I am,

Sincerely yours,

L. G. ROWNTREE, M.D.

Dr. Lewellys T. Barker,
1035 North Calvert Street,
Baltimore

Gohome Bay (via Midland),
Ontario, Can., Aug. 6, 1921.

Dr. H. W. Cook,
Northwestern National Life Ins. Co.,
Minneapolis, Minnesota.

My dear Dr. Cook:

For taking the systolic pressure I think that the palpatory method is satisfactory, though it does no harm to control it where there is the least doubt by the auscultatory method.

In taking the diastolic pressure I think that the auscultatory method is by far the most satisfactory, utilizing the marked change in the sound as the guide (third phase) rather than disappearance of all sounds. I do not think much is to be gained by controlling the diastolic reading by the palpatory method.

The only place where a mistake is likely to be made is in cases of aortic insufficiency, where readings of the diastolic pressure are of practically no value.

You will find my discussion of the whole subject in my article on blood pressure in the Clinical Diagnosis of Internal Diseases (a part of Monographic Medicine) published by Appleton & Co.

Yours sincerely,
Lewellys T. Barker.

The Johns Hopkins Hospital
Winford H. Smith, M.D., *Director*

September 1, 1921.

Dr. Henry Wireman Cook,
Minneapolis, Minn.

Dear Henry:

Your letter of August 1st regarding blood pressure reached me just as I was going on my vacation, so its answer has been

delayed. I use and recommend the end of the third phase for the determination of diastolic blood pressure. I think this point is as a rule easier to read than the disappearance of sounds, and it is definitely shown by Erlang to be more correct. Louis Warfield, I believe, also demonstrated this point. I have not used the palpatory method with the auscultatory. I am not aware of any effort to bring about uniformity in blood pressure determinations or on other examinations. I believe that correct blood pressure examinations can be made by a blood pressure instrument of the spring type, provided such instrument is checked from time to time with mercury manometers and I believe the first appearance of the sounds in the arteries should be taken as a satisfactory point for determination of systolic pressure, while the change from the loud sound to the soft muffled sound is the point at which diastolic pressure should be read.

I suppose you seldom visit Baltimore any more, but if you are here during the next year or two I hope I shall have the pleasure of seeing you.

With kind regards,

Yours sincerely,

G. Canby Robinson,
Acting Professor of Medicine.

Stanford University
Sacramento and Webster Streets,
San Francisco, California.

August 9, 1921.

Dr. Henry W. Cook,
Northwestern National Life Insurance Company,
Minneapolis, Minn.

My dear Dr. Cook:

Your letter of August 2nd, received and I believe the following is a good routine in taking blood pressure. First, take the systolic pressure by the palpatory method. Second, use a stethoscope and read the systolic pressure and then the dias-

tolic pressure as determined by the change in sounds. Third, note when the sounds disappear.

I usually record the systolic pressure as determined by auscultation and the diastolic pressure as determined by the change in sound but I check the former by the palpatory pressure and I check the latter by the disappearance of sound. The palpatory pressure is usually slightly below the auscultatory pressure. When I am taking records for comparison I record, first, the systolic auscultatory and second, the systolic palpatory and finally the diastolic as determined by the change in sound.

Hoping this will answer your question, I am,

Yours sincerely,

A. W. Hewlett.

Western Reserve University,
Cleveland, Ohio.

August 19, 1921.

Dr. H. W. Cook,
c/o *Northwestern National Life Ins. Co.*,
Minneapolis, Minn.

My dear Dr. Cook:

Your letter inquiring about the method which should be employed in studying blood pressure is very difficult to answer, and I do not believe there is any rigid procedure to be prescribed which will succeed in procuring critical knowledge from all examiners. The blood pressure apparatus alone, when uncontrolled by careful criticism, may give a lot of misinformation. I think the point you mention that the examiner should be requested to confirm the maximum systolic point by palpation as well as by auscultation, is very good. I think also that they should confirm the minimum diastolic pressure by palpation as well as by auscultation. Furthermore, the examiner should always estimate blood pressure by palpation of the femoral artery, and if the criticism is to be of any particular value, the size of the aorta and the size of the left ventricle should also be accurately determined.

If any plan is to be adopted which is to be employed by all examiners, I think the confirmation of the maximum and minimum pressures by palpation should be employed. The minimum diastolic pressure should be determined by the point where there is a lessening of intensity of the pistol-shot tone.

Very sincerely yours,

C. F. Hoover.

W. S. Thayer, M.D.
1208 Eutaw Place,
Baltimore, Md.

August 5, 1921.

Dr. Henry Wireman Cook,
Northwestern National Life Bldg.,
Minneapolis, Minn.

Dear Cook:

Your letter comes to hand at a moment when I am in the chaos of moving, and I am going to dictate a few hasty lines.

I've always recorded the minimum pressure at the end of the third phase. It is not always possible to determine this wholly accurately, but it seems to me almost possible. I always control the pressure with my finger and in occasional cases, where the auscultatory method is not wholly satisfactory, I record the pressure taken by the finger with a note to that effect. The trained finger and eye make a satisfactory record.

I'll take your letter with me on my vacation in a few days and think over the other question.

Perhaps you knew that the Pratt Library which owned my dear old house has taken it over this year. At present Miss Smither and I are sitting alone in the new house (which was Russell's) and trying to bring a little order out of the chaos, where the carpenters and paper-hangers come and go.

My greetings to your wife and family and all friends.

Believe me,

As ever,

W. S. Thayer.

Dr. Cook read the following telegram from Dr. J. W. Fisher:

"Your suggestions excellent recording diastolic end third phase, also cessation of sound, solves problem. I move unanimous adoption of report."

Dr. Beckett moved that the report be accepted and that the recommendations be carried out. Dr. Harlow seconded the motion.

Dr. Willard proposed the following amendment to the motion: That the report be accepted and that the matter of carrying out the recommendations in the report be referred to the Executive Council. Dr. Beckett accepted the amendment and the motion as amended was carried.

The Joint Committee presented the following report on the Influence of Family History on Mortality:

REPORT TO THE ACTUARIAL SOCIETY OF AMERICA AND THE ASSOCIATION OF LIFE INSURANCE MEDICAL DIRECTORS, ON THE INFLUENCE OF FAMILY HISTORY ON MORTALITY

One of the questions which the joint committee of the two societies was asked to consider was the Influence of Family History on Mortality. Each of the nine companies represented on the committee was invited to contribute its material for such a study. The larger of the companies were called upon for the data on 1500 cases each, and the smaller companies on 750 cases. As material at the older attained ages was necessary in order to measure the effect of a strong or of a weak family history, it was decided to commence with the policies issued in 1880. Each Company contributed a per-

centage of its experience on the issues for each year from 1880 to 1915 inclusive. These cases fairly represented each Company's experience. From this material all cases in which there was a history of tuberculosis in the parents, the brothers, or sisters were excluded.

The data thus brought together were divided, on the basis of their family histories, into the following five groups:

A. Very Good
B. Good
C. Average
D. Poor
E. Very Poor

After several unsatisfactory attempts had been made by the committee to establish adequate standards by which to determine the group to which each case belonged, it was finally decided to leave the determination to the judgment of the medical director and the actuary of each contributing company. In the course of this work a number of difficult cases were referred to the committee for discussion, and in addition to this, several medical directors conferred on the subject.

The committee measured the data divided into the five classes by the Medico-Actuarial Mortality Table (Select). The result was disappointing as the ratio of actual to expected deaths did not show a smooth progression from Very Good to Very Poor. The officers of two companies offered to revise their experience which showed inconsistent results. Besides this, some additional material was added in order to strengthen the testimony of each group. The results of the second investigation showed hardly any difference from the first.

A member of the committee then undertook to make an independent investigation, using for that purpose certain of the data which were readily available. Each case was rated from $-25+$ to 35 according to a definite rule with regard to the ages at death of the parents, their causes of death, and the ages at death of the grandparents. Where the cause of death was

heart disease, paralysis, or some other disease which according to definition was considered a "blemish," a penalty was charged. In this study, however, the ages and causes of death of the brothers and sisters were not considered excepting in the case of a "blemish." In two of the companies, however, the "unblemished" experience on favorable family histories was quite poor and this section of the material was excluded. This painstaking investigation showed a progression in the ratio of actual to expected deaths from the Very Good on the one hand to the Very Poor family history on the other hand. This progression, however, was so rapid that the committee doubted whether the results were a true expression of the effect of family history or included other factors. It was also thought that the exclusion of part of the material of certain companies had contributed to the results disclosed.

After careful consideration the committee concluded that further work on the investigation was inadvisable at the present time but the members decided to submit to the societies a brief statement of its endeavors and of certain points which were brought to their attention in the course of the study.

In the early days covered by the investigation, family history was looked upon as a far more potent influence than it is now believed to be. In fact, one of the early writers stated that "he looked upon hereditary influence as the first and most important factor in assessing the value of the life of an individual." It was evident from our study that in many instances a very good family history had at that time been considered as an offset to an impairment such as overweight. On the other hand, persons with a poor or a very poor family history had been selected with great care. Under the conditions, therefore, which prevailed twenty or more years ago, the mortality among persons with a very poor family history might not differ materially from that among those with a very good family history, if the selection among the former were severe and among the latter lenient.

To meet this difficulty it was suggested that the cases be reviewed and that all those be eliminated in which it had been

evident that the very good family history was considered as an offset to an impairment of moment or where a very poor family history had been accepted by reason of the peculiarly favorable circumstances in other respects. This procedure would have involved such an amount of labor that the committee did not feel disposed to undertake it, especially as there would be great difficulty in allowing for the more rigid selection in cases with a poor and a very poor family history.

After seriously considering the matter the committee came reluctantly to the conclusion that there was not now available any data in which the same degree of rigidity of selection had been applied without regard to whether the family history was very good, very poor, good, or poor. There is now in existence material of this sort, but it is not more than ten or twelve years old, and accordingly unavailable for use in determining the effect of family history at the older attained ages. The committee therefore decided that the investigation should be postponed until material of a homogeneous nature becomes available.

Dr. Homer Gage, Chairman of the Committee appointed to take up the question of reporting deaths with cause to hospitals, clinics, and surgeons who operated upon the risk, presented the following report:

The Committee to take up the question of reporting deaths with cause to hospitals, clinics, and surgeons who operated upon the risk, beg leave to submit the following report:

To take up each death claim and refer to the cards for impairments, such as previous illness or operation, would not be difficult or very expensive, but in very few instances would there be any record of the attending physician or surgeon or of any hospital residence.

Any illness or operation occurring between the time of application and the filing of the claim would not appear anywhere

in our records. The statement of the attending physician on the claim papers, as a rule, covers only the history of the final illness. Altogether we cannot see how enough information can be obtained through the records of the insurance companies to make the attempt of any value.

Should any large clinic like the Mayo Clinic or any large hospital like the Johns Hopkins in Baltimore or the Presbyterian in New York want us to notify them of any information referring to their former patients, which appears in our insurance record, we think a way could be found to acquaint them with it without great difficulty or expense, but we are afraid such instances would be so few that it would be hardly worth while.

That we can be of assistance to them in following up their cases and determining the final results was established by the work of Mr. Hunter on Duodenal and Gastric Ulcer at the Mayo Clinic. This method of investigation and help is always open to any hospital or clinic that will ask it, and it seems to us the simplest and most logical way in which we can be of assistance. Beyond making known our willingness to coöperate in such investigations as this, we believe no action on the part of the Association is necessary or desirable.

EDWIN W. DWIGHT, M.D.
HARRY TOULMIN, M.D.
HOMER GAGE, M.D., *Chairman.*

On motion by Dr. Beckett, seconded by Dr. Symonds, the report was accepted and the Committee was discharged with thanks. Dr. Willard suggested that the Secretary send a copy of the report to a few of the principal hospitals or clinics, advising them that the companies will furnish "end results" to them if they so desire.

The business submitted by the Executive Council was taken up. The Secretary read the proposed

amendment to Article XII of the Constitution, as follows:

PROPOSED AMENDMENT TO ARTICLE XII OF THE CONSTITUTION

After the words "annual assessment," strike out "of $5.00," and substitute therefor the words, "of $10.00," the amended Article XII to read:

ARTICLE XII

DUES

Each member shall pay an admission fee of $5.00 and an annual assessment of $10.00, payable on the first day of January of each year, and any member who is delinquent in his assessment for sixty days after written notice from the Secretary shall be considered as having resigned his membership. Members having thus lost membership shall, if reëlected within six months, be released from paying a second admission fee.

It was moved by Dr. Symonds and seconded by Dr. Willard that the proposed amendment to Article XII of the Constitution be adopted. Motion was carried.

Dr. Knight—We all of us remember how blue we were when we came together over in the Prudential Building about three years ago. The influenza epidemic was at its height. I wonder if you all know that the Companies represented here paid about 520,000 death claims for $311,000,000 because of the influenza epidemic. When our Executive came to consider the matter, as the epidemic was waning, they realized that with people over the scare, researches were not likely to be continued, money would not be

furnished to the laboratories and to the research men to proceed with the work. This Company got together a Commission of five of the best men they could find in the country to continue studies of influenza and influenza-pneumonia. They secured Dr. M. J. Rosenau of Harvard, Dr. McCoy of Washington, who addressed us over there in the Prudential Building, Dr. Frost of Johns Hopkins, Dr. Jordan of Chicago, and Dr. Park of New York City, and they have been conducting a pretty big work for the past three years. Now the Chairman of that Board, who was selected as perhaps the leading man in this country in that work, has been good enough to come over here to-day and tell us about the results of these studies of influenza and influenza-pneumonia. I take great pleasure in introducing to you to-day, Dr. M. J. Rosenau:

THE INFLUENZA COMMISSION OF THE METROPOLITAN LIFE INSURANCE COMPANY

A Progress Report

By M. J. Rosenau

The Influenza Commission of the Metropolitan Life Insurance Company consists of Dr. M. J. Rosenau, Chairman, Professor of Preventive Medicine and Hygiene, Harvard Medical School; Dr. Lee K. Frankel, Third Vice-President, Metropolitan Life Insurance Company; Dr. A. S. Knight, Medical Director, Metropolitan Life Insurance Company; Dr. W. H. Park, Director, Bureau of Laboratories, New York City Department of Health; Dr. G. W. McCoy, Director, Hygienic Laboratory, U. S. Public Health Service; Dr. W. H. Frost, Professor of Epidemiology, School of Hygiene and

Public Health, Johns Hopkins University; Dr. Edwin O. Jordan, Professor of Hygiene and Bacteriology, University of Chicago. The Commission was organized in the summer of 1919 and is now in the third year of its work.

So far as is known, all the important work on influenza and pneumonia being done in this country is either under the direct supervision of the Commission or is correlated with it. This unusual example of the mobilization of work done in many different laboratories widely scattered is unique.

It is the unanimous opinion of all the members of the Commission that little or none of the work being done would have been undertaken without the support of the Metropolitan Life Insurance Company. Furthermore, this help has not only stimulated effort, but has been the means of obtaining supplementary appropriations that otherwise would not have been made. For example, the University of Chicago, Harvard University, and Johns Hopkins have each made additional appropriations or allotments to supplement those of the Metropolitan Life Insurance Company, and have furthermore placed material, personnel, and other resources at the disposal of the Commission.

We have also enjoyed active coöperation with the United States Public Health Service, with several state boards of health, notably Massachusetts, New York, and Maryland, and also with various city health departments, hospitals, laboratories, and other agencies. Not a single request for coöperation has been refused and our relations with all these various agencies have been cordial and helpful.

The forces of the Commission are now working upon the problems of influenza and its chief complication, pneumonia, in Washington, Baltimore, New York, Boston, and Chicago. The Commission has been at work a little over two years and it now has a "running start" in that each member of the staff has organized and trained workers specially skilled for the work in hand.

I would be remiss were I not to express the appreciation of the members of the Commission to the Metropolitan Life

Insurance Company for the opportunities for advancing research in our respective laboratories. Dr. Knight and Dr. Frankel have been active members of the Commission and have had a clear comprehension of the problems and a sympathetic understanding of the difficulties of work of this kind. The idea was initiated by Dr. Frankel and was met with enthusiasm from the beginning.

Work of this sort is cumulative. This statement should be regarded only as a report of progress. A brief outline summarizing the work of the individual members of the Commission follows.

A SUMMARY OF THE RESULTS OBTAINED BY THE INVESTIGATORS AT THE RESEARCH LABORATORY WORKING UNDER THE INFLUENZA COMMISSION

By Dr. William H. Park,

Department of Health, New York City

The great pandemic of 1918 had subsided but the fear of a recurrence in the fall of 1919 was in the minds of all.

The Influenza Commission believed the most pressing problems were the solving of the riddle as to the identity of the microörganism primarily responsible for the pandemic and of the principal microörganisms causing the complications, which often seemed to be more important than the primary disease.

If this knowledge could be obtained, other problems, such as the use of vaccines, the degree of immunity conferred by an attack, the effect of crowding and of housing conditions in general, and the means by which the infection was spread and how it could be prevented, could be investigated with greater hope of obtaining valuable information.

It seemed to the Commission that it would be wise for me and those associated with me to study at first the relation of

the influenza bacillus to the epidemic and the effect of vaccine composed of influenza bacilli alone and with pneumococci. If neither of these germs had a causal relationship they were at least the most frequent cause of the complicating and idiopathic pneumonias.

The report of Pfeiffer in 1893 on the rôle of the influenza bacilli in the pandemic of 1891 had caused its immediate universal acceptance as the probable primary factor. The gradual discovery that it was present in the respiratory passage of many showing no symptoms of influenza but suffering from other diseases and also in those of normal persons led some to the idea that it might be a secondary invader in influenza instead of the primary cause. In spite of this doubt the influenza bacillus was at the beginning of our work considered by the great majority of investigators as at least the most probable cause.

Our first problem, therefore, was to solve the relation of the influenza bacillus to the epidemic disease. As we improved our technique we found the bacillus in practically every case of influenza. We also found it almost as often in cases of whooping cough, measles, and common colds. We noted that the different cases very frequently gave bacilli differing from each other in essential characteristics. The strains differed for instance in their action on certain sugars and in the production of indol. The finest point of difference, however, was in the effect that the serum antibodies of an animal which had been injected with one strain would have on it and on others.

The serum from such an animal would, on having been mixed with the strains of bacilli used to immunize it, cause the bacilli to clump together. On other strains that were alike the serum would have the same effect, but on those that were different it would produce no clumping. We tested in many ways the accuracy of this test. Cultures taken at different times from the same case gave the same strain of bacilli. These resembled each other in every way. Cultures grown for months in test tubes kept their characteristics practically unaltered.

The obtaining of the knowledge that there were many

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different strains of influenza bacilli and that these strains differed in characteristics which could be studied and that these characteristics were stable at least for many weeks, that is, strains which were different did not change so as to become identical, gave us a new means of solving the relation of the influenza bacillus to influenza.

It may be well to develop this idea a little through analogy before stating it technically.

Supposing trucks run back and forth across the middle of some fields parched by drought.

Some of the trucks carry winter wheat and some water. We notice soon that between their tracks blades of wheat spring up. We think that the trucks have dropped some grains of wheat and that the wetting of the soil by the leaking of water allowed them to grow. Soon, however, there is abundant rain and to our surprise we find wheat springs up everywhere in the fields under observation as thickly as between the tracks and in our minds doubt arises as to whether the trucks dropped any wheat. This leads to closer study and we find that the growing wheat is of the spring and not the winter variety.

Now we are sure that the wheat was already in the ground and that it did not fall from the trucks.

We supposed to ourselves that if influenza was spread by the influenza bacillus and there were different strains of bacilli just as there were different strains of wheat and that these strains had persisting differences then a case of influenza disseminating the bacilli to a group of school children or a boatload of passengers would produce the disease in each by the development of its own kind of bacilli. If other kinds of influenza bacilli appeared and these were of several kinds then one would realize that mucous membranes of the people had been carriers of the bacilli just as the field carried the spring wheat and that some contagion had in lessening the resistance of the mucous membranes prepared a soil for their growth just as the water had prepared the earth for the sprouting of the spring wheat. The proof that the bacilli were obtained from different persons who had contracted the disease was quickly obtained.

But this was insufficient evidence. Our problem was rendered more intricate because of the fact that we recognized that nearly every person was a carrier of influenza bacilli so that it was possible that when the virulent strain was passed from the sick to the well and developing in their throats gave them influenza that the old less virulent dominant carrier strain might pick up strength and grow also in the inflamed mucous membrane.

From reasoning and experience we thought that it was safe to assume that the invading virulent strain would at least be prevalent and probably in marked preponderance on the surface of the inflamed mucous membrane. With this point of view we cultured some 200 persons who had just developed acute influenza and we found after diligent search no trace of any common strain of any microörganism.

We felt it, therefore, correct to assume that some unknown virus had lowered the resistance of the respiratory tissues and made them susceptible to the growth and toxic action of the influenza bacillus. It was in short one of the germs that frequently produced more or less serious complications but was not the primary cause. This conclusion was reached at the conclusion of the first year's work.

We then took up the varieties of both the more hemolytic and the less hemolytic streptococci which were thought by some investigators to be the cause of influenza. These like the influenza bacilli were found abundantly present and to differ in the strains obtained from the different individuals selected from those infected from the same source and attacked at the same time. The pneumococci were also investigated in the same way and with the same results.

The important groups of streptococci and pneumococci must therefore be considered as important secondary invaders in and not as causative organisms in influenza. They have the same relationship to epidemic influenza as the influenza bacilus.

We feel confident at the close of these investigations that no one of the well-known types of *bacteria that* have been suspected

is the cause of influenza. In order to make our knowledge more complete we studied the microörganism in a number of healthy throats of a number of persons and then when any of these developed colds or influenza we again took cultures. The results indicated that the same strains of microörganism were present in the cultures as were obtained from the same cases after they contracted influenza.

If we are asked what is the value of these results obtained with so much labor we answer that in demonstrating that neither the influenza bacillus nor any other of the well-known respiratory germs is the cause of influenza we have prepared the ground for the search for new microörganisms of the less easily cultivatable kind and specially for the filterable viruses such as the one isolated by Olitsky.

We have also added greatly to the knowledge of the great multiplicity of strains or types among the different varieties of bacteria causing the ordinary respiratory infections and thus made clear the great difficulty of developing a composite vaccine that will be a preventative of all respiratory diseases.

VACCINATION AGAINST PNEUMONIA AND COMMON COLDS

Through the coöperation of Drs. Knight and Frankel we were able to obtain 1500 volunteers among the Metropolitan employees. They received three injections of a vaccine composed of six billion of the most frequently found germs in acute respiratory infections; namely: influenza bacilli, 1 billion; streptococci hemolytic and non-hemolytic, 1 billion each and the three most usual types of the pneumococci, 5 billion. There were 3000 untreated employees who were watched as controlled.

We kept a record of the reactions to the vaccines given, partly for general scientific knowledge and partly for the purpose of finding out the practicability of group vaccination for a commercial company from the point of view of loss of services following and due to the inoculations. The injections were given subcutaneously in the deltoid region. The first dose was 0.5 cc. and the second and third were 1 cc. each.

There were, following the first injection, mild local reactions (area of redness not more than a silver dollar in size, and very slight soreness) among 63.8 per cent. of the group; moderate local reactions (area of redness from size of a silver dollar to size of hand and slight soreness) in 11.8 per cent. of group; marked local reactions (anything larger than moderate) in 3.3 per cent. of the group and no reaction at all in 21 per cent. Following the second injection (double the dose) mild local reactions were observed in 63.2 per cent. of the group, moderate in 21.4 per cent., marked in 2.7 per cent. and no reactions in 12.7 per cent. Following the third injection, mild local reactions were seen in 60.6 per cent., moderate in 9.7 per cent., marked in 4.1 per cent., and none in 25.5 per cent.

Of the general reactions, the mild ones, consisting of headaches, slight dizziness, nausea or general malaise were reported after the first injection, in 19.4 per cent. of the group; after the second injection in 27 per cent. and after the third in 23.7 per cent. The marked reactions, chills or chilly feelings, fever, general pains, or mild prostration, after the first injection, were observed in 3.1 per cent.; after the second, in 3.2 per cent. and after the third, among 2.6 per cent. Only 26.2 per cent. of the total inoculated gave general reactions. These figures show that the largest number of reactions, as was to be anticipated, followed the second injection and that the severe reactions were comparatively few.

A tabulation was made to learn whether there was any demonstrable relationships between past respiratory diseases, such as pneumonia, bronchitis, grippe, etc., and the character and severity of the reactions from the inoculations. No especial susceptibility to vaccines can be established for these diseases.

RESPIRATORY AFFECTIONS AMONG THE VACCINATED AND UNVACCINATED DURING THE SIX MONTHS OBSERVATION PERIOD, OCTOBER 1, 1919, TO APRIL 3, 1920

There were 1327 completed records of inoculated persons under continuous observation during the above period and

3025 similar records of the uninoculated. Among the inoculated, 13.7 per cent. give a history of no respiratory affections during this time. Among the non-inoculated, 29.77 give a similar history.

One might infer from the tabulated figures that it were wiser not to be inoculated. One must remember, however, that the observed groups were volunteers. Naturally, those volunteering for inoculation might be supposed to be the more susceptible to respiratory diseases, hoping for relief from the inoculations. And, in fact, our summaries of the original questionnaire show that 21.6 per cent. of the non-inoculated were comparatively free from respiratory troubles in their past, as against 10.5 per cent. of those who submitted to inoculation.

It is probably fair to conclude that, considering the results as a whole, the kind of vaccine used by us, in the manner and dosage indicated, had very little specific influence either in preventing or causing the milder respiratory diseases that is those other than pneumonia. The result of our investigations which had emphasized the multiplicity of germs capable of causing the milder respiratory infections had made us skeptical of any marked benefit from vaccine in these cases. With pneumonia the conditions are different. This disease occurs in two fairly well differentiated varieties; lobar pneumonia and broncho pneumonia. The first is almost always caused by the pneumococci and the second frequently. We had because of these facts and of some earlier work by others, hopes of results from the pneumonia vaccine.

PNEUMONIA

The difference of pneumonia incidence, was marked in the two groups during the first six months.

The inoculated showed 1 case (0.075 per cent.) against 11 cases (–.36 per cent.) among the non-inoculated, or about five times as many pneumonias among the non-inoculated.

It is true that the number of cases of pneumonia occurring

in the group is so small that the apparently favorable results of vaccination may be due to chance, nevertheless, it would seem fair to make some claims for the efficacy of a fixed type pneumococcus vaccine as a preventive of pneumonia. The one case of pneumonia which occurred in the inoculated group, was a girl who had had only one injection of the vaccine on October 2, 1919. She suffered from influenza and bronchopneumonia March 23, 1920, more than five months after vaccination. She was absent from her work just one month. She had had an acute bronchitis (absent 20 days) in January, 1920.

Lister in South Africa does not claim much immunity resulting from the pneumococcus vaccine for more than six months. After our investigation had been closed, two more cases of pneumonia were reported among the inoculated. Both occurred in May, 1920, about seven months after the vaccination. One of these patients had had two and the other three injections.

In the fall of 1920 we vaccinated 700 employees of the New York Telephone Company and 2000 children. The results in these cases are reported as equally favorable. We are also testing very thoroughly various methods of preparing vaccines, so as to use the most effective vaccines in future attempts at immunization.

Our conclusion is that we have in typed pneumococcus vaccine a preventive measure of considerable value. Further observations are necessary before it will be possible to decide whether it is sufficiently effective to urge upon people for general use. It is a decided drawback that it is not usually effective longer than six months. Pneumonia is such a fatal disease that even this objection would be brushed aside, if we felt certain that the vaccinated had considerably less pneumonia than the unvaccinated.

During the spring and summer we have been studying some of the filterable viruses with the object of familiarizing ourselves with their peculiarities. We intend in October to take up the investigation of the virus isolated by Olitsky from

several cases of acute influenza and thought by him to be the causal agent. The fact that this virus has been isolated from influenza cases and when inoculated into animals intratracheally causes an inflammatory reaction similar to that seen in man is most interesting but does not prove its causal relationship.

REPORT OF WORK DONE AT THE UNIVERSITY OF CHICAGO UNDER THE SUPERVISION OF DR. E. O. JORDAN

At the University of Chicago observations were made on the existence of immunity to influenza, the results of which have been published in the *Journal of Infectious Diseases*. Apparently no marked immunity to influenza exists twelve to fifteen months after a previous attack, although it is possible that some degree of immunity may be obtained at an earlier period.

An extensive series of observations has been made in certain state institutions of Illinois on vaccination against respiratory disease. A widely used vaccine was employed containing certain varieties of streptococci and pneumococci as well as the so-called influenza bacilli. About 6000 persons were under observation during a period of seven months. Approximately one-half of this number received the vaccine. Ordinary cases of colds and bronchitis developed with about equal frequency in vaccinated and unvaccinated groups. Influenza attacks amounted to 4.1% among the vaccinated and 4.8% among the unvaccinated. This difference is not statistically significant.

Bacteriological work on respiratory infections has been carried on according to the comprehensive plan adopted by the Respiratory Disease Commission. Extensive data have been accumulated on the occurrence of microörganisms in the respiratory tract in health and in disease, and these are now being tabulated and studied. A special study of an unusual outbreak of respiratory disease among the pupils of the University School of Education has extended over several months.

Studies have also been made on the possible occurrence of a filterable virus as the cause of common colds and influenza, but so far without positive results. Special studies have been made of the relation of the pneumococci to acute infections of the upper respiratory tract, and a similar series of observations on the Gram-negative cocci in colds and influenza is ready for publication.

REPORT OF WORK DONE UNDER THE SUPERVISION OF DR. G. W. McCOY

Director, Hygienic Laboratory, Washington, D. C.

PNEUMONIA

1. *Field Vaccine Experiments*

Approximately 25,000 persons were vaccinated in state hospitals in New York and Massachusetts, using, in the case of New York, an oil vaccine which had acquired large repute in the American Army, while in Massachusetts the ordinary saline suspension was used. In each case an equal number of persons were kept under observation as controls. The experiments at first appeared to indicate that a considerable degree of protection had been conferred, but a closer study of the figures showed that this was incorrect and that at best a very moderate reduction of mortality and morbidity had been brought about—a reduction, even if substantiated, too small to warrant the general advocacy of this method of prophylaxis.

2. *Laboratory Vaccine Experiments*

A group working at the branch of the Hygienic Laboratory at Bellevue Hospital in New York City under the direction of Dr. Russell L. Cecil, consisting of one physiological chemist and one bacteriologist, has devoted its attention to the preparation of a vaccine against pneumonia which would be effective and sufficiently devoid of unpleasant consequences following injection to make its use on a large number of persons possible. A large number of the derivatives of the pneumococcus have

been studied, but none so far produced offer any special grounds for encouragement.

3. *Etiology of Pneumonia*

The cases of pneumonia which have occurred at Bellevue Hospital within the past year have been studied etiologically, i.e., the causative organism has been identified where possible and a comparison made with similar studies carried on in previous years. It would appear that the bacterial causes of pneumonia are now similar to those before the influenza epidemic which introduced so many new factors that the percentage prevalence of different organisms was different from that in normal times.

4. *Therapeutic Experiments*

A rather promising preparation made from antipneumococcic serum has been used for the treatment of a large number of cases under Dr. Cecil's direction. The results thus far have been distinctly encouraging, but it is not desired to make any statement as to the value of the preparation until at least 200 cases have been treated, and a similar number under like conditions but not treated by this special preparation, have been observed as controls.

An experimental basis for the serum treatment of pneumonia has been established through experiments with monkeys, certain species of which are relatively easily infected with the pneumococcus.

5. *Attempts to Produce Influenza*

The crucial point as to the relation of the Pfeiffer bacillus to influenza was subjected to experimental test by the inoculation of volunteers with fresh, virulent cultures of this organism; while the results were not so clear-cut as might be desired, it may safely be said that influenza was not produced. This is in keeping with other attempts which have resulted in failure, where an effort was made to infect man by means of secretions from the air passages of cases of influenza.

6. *Pathology of Pneumonia*

Working under the direction of Professor Wm. B. Wherry at the University of Cincinnati, Dr. C. A. Mills has studied a number of the factors involved in the consolidation of the lungs in pneumonia. Dr. Mills' work has not been carried far enough to lead to any definite conclusion, but he has undoubtedly made some substantial contributions to the whole subject of blood coagulation.

MEMORANDUM ON STATISTICAL AND EPIDEMIOLOGICAL STUDIES OF INFLUENZA AT BALTIMORE AND ELSEWHERE

By Dr. W. H. Frost

To give a true conception of the causes, means of spread and possible control of any disease, experimental studies must be combined with exact knowledge of its characteristic natural occurrence; and the Influenza Commission has naturally included epidemiological studies in its program of investigation of influenza. In this as in other lines of investigation the Commission has preferred not to undertake independent studies, but rather to strengthen the organizations already in the field, in this case a group of workers in the Public Health Service, with headquarters in the Statistical Office of the Service in Washington, where all the facilities for statistical research are already available.

This phase of the work has been assigned to Dr. Frost, one of the members of the Commission, who, as an officer of the Public Health Service, is in charge of the epidemiological studies of influenza which that Service has been conducting since the outbreak of the epidemic in the fall of 1918. Associated with Dr. Frost is Mr. Sydenstricker, the chief statistician of the Public Health Service, in charge of their statistical office. By the grant of funds sufficient to pay the salaries of an assistant statistician and some statistical clerks, the Com-

mission has made it possible for the Public Health Service to complete its statistical studies of influenza which otherwise must have been abandoned, or at least seriously curtailed, on account of lack of funds.

The support of the Commission has also been given to some statistical studies of influenza mortality which are being carried on at the Johns Hopkins University, School of Hygiene and Public Health, by Dr. Raymond Pearl, Professor of Vital Statistics and Biometry.

The chief work undertaken originally by the Public Health Service and continued with the aid of the Commission, has been a thorough analysis of the records of extensive surveys made during the winter of 1918–19 in twelve localities in various parts of the United States, to determine exactly the rate of prevalence of influenza, and its relation to color, sex, age and other conditions of life. Strange as it may seem these surveys, covering more than 150,000 people, constitute the only exact record of this kind which is as yet available, since previous epidemics were not studied in this way, and so far as is known, no other surveys of comparable extent were made elsewhere in the 1918 epidemic.

Two similar surveys, both supported by the Influenza Commission, were made following the epidemic of 1920, one in Baltimore by the Public Health Service, and one in Boston, by Dr. Vaughan, under Dr. Rosenau's direction. These two surveys, which, though made independently, agree in all essential respects, furnish a very satisfactory basis for comparison of the 1918 and 1920 epidemics.

Other lines of study carried out by the Public Health Service in connection with these special surveys are an extended analysis of statistics of mortality from pneumonia and influenza in the United States in the interval from the epidemic of 1889–92 to the present time, and the compilation of reports from foreign countries.

The studies made from this angle of statistical and epidemiological analysis have already been published in:

(1) An extensive monograph by Dr. Vaughan, reviewing the

whole history and epidemiology of influenza in connection with his studies in Boston.

(2) A number of papers by Dr. Frost, Mr. Sydenstricker and others, issued mostly in the Weekly Public Health Reports of the Public Health Service. These separate reports are now being combined into one comprehensive report of the studies carried out by the Public Health Service.

(3) A series of papers by Dr. Pearl, likewise published in the Weekly Public Health Reports, and to be extended when he has completed the work now in progress in his laboratory.

As to the significance of these studies in contributing towards such knowledge of influenza as will eventually bring the disease under control, they are of value chiefly, perhaps, as affording a more solid and extensive foundation of facts upon which to build in the future. It is doubtless impossible at present to give a correct interpretation of all the facts which have been established; but the establishment of these facts at least marks the completion of an essential preliminary stage of investigation. There are, however, certain facts brought out by this line of investigation, which already have a very definite if not very encouraging significance in relation to control of the disease, for example:

1. It is now clearly shown that persons who have been attacked by influenza are not immune from further attacks after twelve to eighteen months. This reduces the hope of effectively controlling the disease by vaccination against it.

2. It is also shown that the sick rate and the death rate from influenza are not by any means parallel. We may have a high attack rate and a low death rate and vice versa. This indicates that we must look further for the particular conditions which contribute to a higher fatality and that if we can control these we may be able to greatly decrease the death rate even if we cannot control the attack rate.

3. A further significant fact brought out by our studies is that the epidemic of 1918 was not of such sudden development as it appeared to be, but was preceded at least in the United States by a definite increase in pneumonia deaths, occurring

almost simultaneously throughout the country in the spring of 1918, and less definitely in 1916 and 1917. A closer study of pneumonia mortality and more attention to local outbreaks of "grippe" should forewarn us of the next epidemic.

4. Finally, these studies have added to the conviction that influenza even in the intervals between great epidemics is a factor of importance in the death rate from ordinary pneumonia; and have emphasized the necessity of studying the pneumonias continuously, not only in periods of epidemics.

REPORT OF WORK DONE AT HARVARD UNDER THE SUPERVISION OF DR. M. J. ROSENAU

Much of the mystery and misconception concerning influenza is due to a lack of exact records of previous epidemics and of interepidemic periods. The Commission has, therefore, considered it highly important to compile and publish exact records of the recent outbreaks for the sake of our present purposes as well as for future use. Studies of this kind were, therefore, set afoot in a number of different places directed by the Public Health Service at Washington, Dr. Frost in Baltimore and by Dr. Vaughan in Boston.

Dr. Vaughan's monograph on "The Epidemiology of Influenza" has just appeared as No. 1, Monograph Series, *American Journal of Hygiene*, July, 1921. It is based upon a careful study of 10,000 cases in Boston. It is difficult to summarize the chief features of this monograph, which is a distinct contribution to the subject. It includes both the medical and sociological features of the disease. All factors such as race, age, sex, occupation, crowding, etc., are fully considered and discussed. Dr. Vaughan not only considered influenza as it affected the individual and community, but considered it from the standpoint of the family.

The monograph is in eight sections as follows: Section 1—Review of the literature between 1893–1918 and shows that the disease has been prevalent in one place or another throughout that time. Section 2—The pandemic spread is described with

the possible source of origin in the United States although it is known that influenza existed in France and China as early as it did in this country. Sections 3, 4 and 5 deal specifically with the work done in Boston which is correlated with the findings of other investigators. Section 6 deals with immunity. Section 7 discusses the influence on other diseases, and the final Section 8 includes recommendations for the future.

Immunity

Our studies in Boston comprising 10,000 persons confirmed the findings of Jordan and Frost. It seems clear that one attack of influenza does not protect against subsequent attacks—as is the case with smallpox, measles, typhoid fever and many other diseases. The immunity following an attack of influenza is transient and brief. There is evidence to indicate that there is some protection, which wears off in about a year.

Vaccines

A large number of persons were vaccinated in Massachusetts state institutions with the usual controls. These observations convince us that the vaccines as used do not protect against influenza. The results of vaccination against pneumonia are uncertain and not particularly encouraging.

Bacteriology

Bacteriological work upon the cause of influenza is being done in each laboratory of every member of the Commission. It required a great deal of work to convince us that the so-called influenza bacillus of Pfeiffer is not the true cause of influenza, although it is often a secondary invader and thus aggravates the disease or produces complications.

Secretions were selected by Dr. Park from a number of persons having colds and filtered through stone filters. The filtrate was inoculated into the nostrils of forty students, but except in two cases the students remained well and in the two cases that did develop coryza, some ordinary cause was prob-

ably at work. Similar studies upon volunteers in Boston under Dr. Rosenau's direction also proved negative.

Pneumonia

Pneumonia being the cause of death in influenza, the Commission is devoting much time and attention to its prevention and treatment. In Boston, Dr. Moss and his staff in Dr. Rosenau's laboratory are making a special study of the crisis in pneumonia. For this purpose the Boston City Hospital has arranged a special pneumonia service of about 50 beds which have been put at our disposal. All the protective mechanisms of the body are being studied systematically upon these cases, such as phagocytosis, agglutinins, precipitins, complement fixation, bacteriotropins, bacteriolysins, immune bodies and skin anaphylaxis.

Pneumococcus Vaccines

This work is being carried out in the institutions for the insane throughout the Commonwealth of Massachusetts. The work correlates with similar observations made in the State of New York. At first there seemed to be good evidence that the vaccine against pneumonia due to type 1 was fairly effective, although for a short time, but further analysis of the figures throw doubt upon this. We are now inclined to believe that the work so far as it has gone has not demonstrated any prophylactic value following vaccination with pneumonia vaccine.

Anaphylaxis

Dr. Moss and his assistants are studying the serological reactions in cases of pneumonia before and after the crisis, at the Boston City Hospital, and other features of the disease both clinically and bacteriologically.

Professor Bronfenbrenner and Dr. Rosenau are studying the crisis and lesions of pneumonia with reference to anaphylactic reactions.

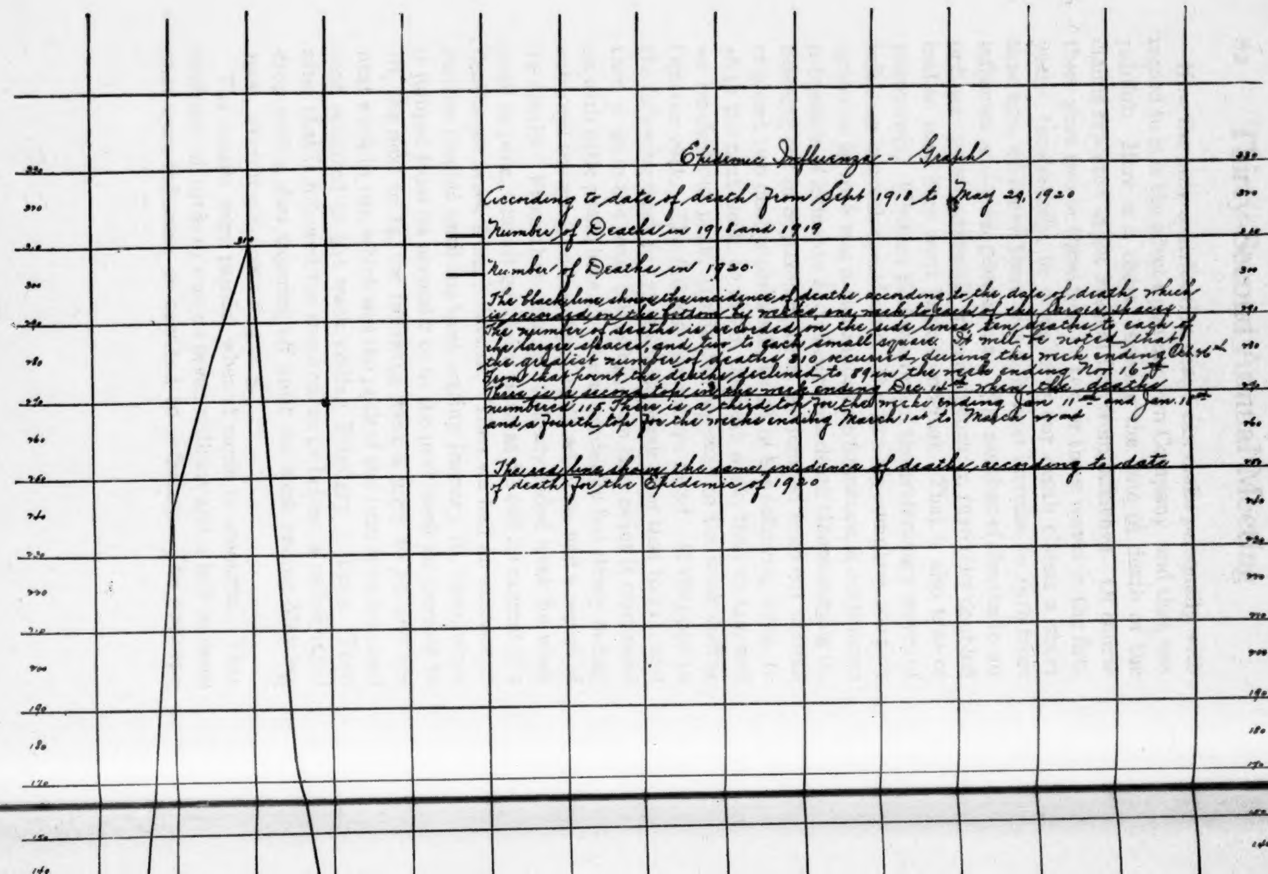
Dr. Knight—Anybody who would like the gift of one of these monographs of Dr. Vaughn from the Metropolitan may have it mailed to him, if he will leave his name. I knew you would all be glad to hear Dr. Rosenau because we are all doctors as well as Medical Directors, and I think he was very kind indeed to come and talk to us as he has done. Dr. Symonds will open the discussion.

DISCUSSION

Dr. Symonds—Mr. President, Dr. Rosenau: I want to thank Dr. Rosenau personally for what he has told us to-day. It has been a very interesting contribution to a matter which touches life insurance closely. I think you said $311,000,000 of death claims were paid because of the epidemic, and the only regret I have is, that Dr. Rosenau was promising, but not hopeful—he promises another epidemic before long, but he is not very hopeful about the prospects of our being able to combat it. It is of course in fact a serious matter because our rates of premium are based entirely upon the experience in an inter-epidemic period. The old American Experience Table was prior to the epidemic of 1889. The Medico-Actuarial and the American Men's Table was based upon the experience in between the two epidemics, and does not take any account at all of the epidemic, and if we are going to have an epidemic every thirty years or thereabouts, our actuaries should take it into account. This is certainly something that we must figure on, —when the mortality rate of a company jumps from 60 to 90% in a year's time, we suffer very much. I hope that the Commission will continue in its work. It is a very great idea, and I think that the country as a whole owes a great deal to the Metropolitan Life Insurance Company for assisting and helping to coördinate this work. It is something that they do and have done repeatedly in other directions—a very generous, wide-spirited act with large vision.

Now the only contribution that I can make personally with regard to it is the effect upon my own Company, and that was painful. Here is a chart, showing the date of death of the claims reported to the Mutual Life from influenza. Of course there were two or three tops or two or three waves in the first year. Incidentally, in going over our death claims a short time ago, we found there was a distinct increase in 1916 from influenza and from pneumonia. The number of deaths in an ordinary period are small, from influenza; in 1916 they doubled and in 1917 they went back to normal. That is also true of pneumonia. Whether that was one of the preliminary waves of influenza is worth considering. But in a big company, the first epidemic in 1918 was a continuous performance, a continuous procession of influenza and pneumonia death claims coming in. Starting out in September, 1918, we jumped from our normal of about two deaths per week, to 18 in the following week, to 46 in the next week, to 118 in the next week, then to 242, and we reached our peak, 310 deaths reported in the week ending October 26th. The fall was even more rapid. It dropped in the following week to 177, then the week after that to 111, and then to 90 in the week following; from that point it continued on with little peaks here and there, gradually but slowly fading out until the week ending April 19th, when we had a report of 12 deaths. From that time on it slowly faded week by week until in June, when it got down to what we call its normal of 2 deaths per week from influenza. Then we had an absence of further trouble until the week ending January 10, 1920, when it jumped from its normal 2 to 6; the next week an increase to 10, the next to 14, the following week a jump to 45, and the next week to 102, which was the peak of the 1920 epidemic, and that occurred in the week ending February 7, 1920. Then after that it followed the same curve as before, a rather rapid drop with a slow tapering off until the week ending May 29, 1920, when it got down to 4 or 5.

The results were painful when it came to amounts. That epidemic of 1918–19 cost us seven millions and a half in cases that were influenza or ascribed to influenza. The 1920 epi-


Epidemic Influenza - Graph
According to date of death from Sept 1918 to May 29, 1920
Number of Deaths in 1918 and 1919
Number of Deaths in 1920
The black line shows the incidence of deaths according to the date of death which is recorded on the bottom by weeks, one week to each of the larger spaces. The number of deaths is recorded on the side lines, ten deaths to each of the larger spaces, and two to each small square. It will be noted that the greatest number of deaths 310 occurred during the week ending Oct 26th. From that point the deaths declined to 89 in the week ending Nov 16th. There is a second top in the week ending Dec 14th when the deaths numbered 115. There is a third top for the weeks ending Jan 11th and Jan 18th and a fourth top for the weeks ending March 1st to March 22nd
The red line shows the same incidence of deaths according to date of death for the Epidemic of 1920
320
310
300
290
280
270
260
250
240
230
220
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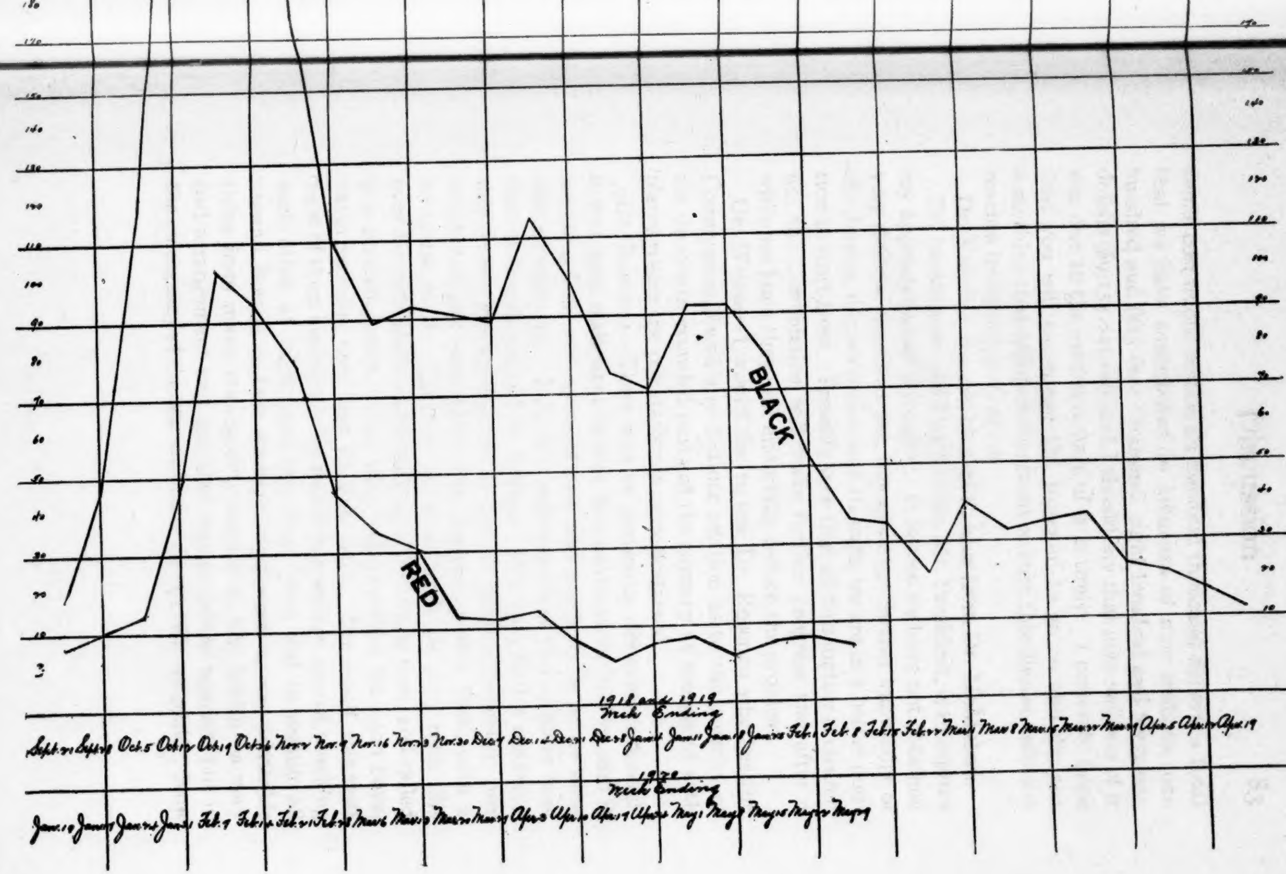
180
170
160
150
140


BLACK
RED
1918 and 1919
Week Ending
1920
Week Ending

demic cost us one million six hundred thousand dollars, a total that we have contributed to influenza of nine millions, one hundred and fifty-four thousand, eight hundred and forty-one dollars ($9,154,841.00), and I should say that nine millions of it was due to the epidemic part of it entirely. I certainly hope that you will encourage Dr. Rosenau to go on and dig out something that will either prevent or cure this disease before it reaches its fatal termination.

Dr. Knight—We will be glad to hear from Dr. McMahon.

Dr. McMahon—All I wish to do, Mr. President, is to express my appreciation of the paper. It is true we have not obtained very definite results as yet, but as we get further knowledge of the disease, its prevention, and its cure, we are in a better position to start from. Possibly now that all this matter is cleared up, the Commission will make further progress and after a while we hope they will materially reduce the problem.

Dr. Weisse—I would like to ask Dr. Rosenau whether the Commission found any distinct relation as to the severity of the disease in crowded parts of the country as compared with places where the population is more scattered.

Dr. Rosenau—There was no particular difference although it was most malignant in that first outbreak in Boston, and it waned a little as it traveled west and south, as far as we were able to make out. I think it might interest you to know how that outbreak started in Boston. The navy had a receiving ship which was nothing more nor less than a steamship pier which had just been built—the Commonwealth Pier—with a structure on it. The war broke out and the navy took this over for mobilization and training of its young men, and called it a receiving ship. That pier had provision for 2400 boys, naval recruits; they sent us 5000 boys. The result was that 2400 of them swung side by side in hammocks, almost touching each other at night, and the other 2600 had to scatter all around Boston to find places to sleep—not a very advisable thing from many standpoints outside of just health—a very bad arrangement and one that was protested against, but in the emergency of the war we had to do the best we could. Now

that was where the first outbreak came, and it was very clear to us that similar types of influenza came over on some of the ships that came to Charlestown Navy Yard, and one of them got into this Commonwealth Pier. Of course it spread very rapidly among those boys, and every night 2600 of them scattered this infection broadcast throughout metropolitan Boston, and there never was such an explosion of such explosive violence of any disease in the history of the world. This started August 27th—that was the first case as far as we could make out—and before two weeks were over, the whole city was aflame, the hospitals were overcrowded, temporary barracks also, almost to the disabling of the community. I think that little bit of history might be interesting to you, as showing the way it started in this virulent form in the second wave.

Dr. Blakely—I wonder if Dr. Rosenau would not tell us a little bit more about that man in the Peter Brigham Brent Hospital who scattered the influenza so generously.

Dr. Rosenau—It wasn't a man. It happened to be a woman! There is nothing that we can say, except that it appeared to be an ordinary case clinically, and we could find no difference between that and the other case, excepting this epidemological fact, that persons coming in contact with that particular individual contracted the infection. In Dr. Vaughn's monograph you will see that you get a case of influenza in the height of an epidemic in a family, with eight or ten in the same house perhaps, and that nobody else gets the disease in that house from that case, whereas you know that ordinarily in a family outbreak, within a week almost everybody has it. Now I have purposely tried to be conservative and not give you false hope, because really the problem is still to be solved.

Dr. Willard—Have your statistics been studied as to the distribution of the disease as to ages, Dr. Rosenau? The reason I ask that question is this: As we were paying death claims at the Home Offices of Life Insurance Companies, it did seem as though from just the facts submitted to us, that people between the ages of 18 and 40, we will say, contributed a larger proportion of the deaths than relatively their share, in com-

parison with the total number; in other words, that the fatal evidences of the disease seemed to show itself largely in those who are in the central period of life activity and presumably the healthy people of the family. The extremes of infancy and of old age, in the homes where death occurred, seemed to be spared, but two or three and sometimes four of the young, vigorous adults would die, while the extremes of age on either side were untouched.

Dr. Rosenau—That is very true, with this exception, that while the greatest death rate was at that period the greatest incidence was among children, but it was not so virulent in children. It took the strong and robust, and particularly pregnant women, where it had an unusual virulence. That was a new manifestation. We have not been able to find that particular fact in previous outbreaks.

Dr. Toulmin—Do you trace this and other epidemics back to some central point in the Far East?

Dr. Rosenau—We did hear that the home of influenza was in Turkestan, but I think that was because no one knows anything about Turkestan, or even where it is! That is a very important thought, for if influenza, like the bubonic plague, has an endemic home, and an endemic focus, if we could find it, then it would not spread every once in a while to all parts of the world.

Dr. Harlow—In looking over the Public Health reports of the influenza epidemic, I found that there were three cities that had the lowest mortality in the United States. These were Milwaukee, Minneapolis, and Kansas City, and not only did that mortality show an unusually low death rate from influenza, but, if I am correct, in previous epidemics of influenza that was true also, yet I have never been able to get a satisfactory explanation of why that is so.

Dr. Rosenau—I do not carry that in my mind and I would not be able to answer the question. The only correlation that remains with reference to this question is, that the cities which used the most aggressive measures, as to closing the schools, department stores, movies, and about people riding on the cars,

etc., seem to have been hit the hardest, and cities where the health officer rather let things go did not have it quite so bad.

Dr. Harlow—Milwaukee was not in the class referred to by Dr. Rosenau in that way, because we had a very efficient health officer there.

Dr. Scadding—Dr. Rosenau spoke of immunity lasting for a year. It is generally held that immunities last much longer than that, that those who had the attack in the previous epidemic seemed to be immune to a large extent from attacks in the last epidemic. As to the mortality, in the first epidemic the mortality was largely among the people between 50 and 60, and in this epidemic among the people in the thirties.

Dr. Rosenau—I based my statement on the fact that 6000 cases were studied among the personnel of the Great Lakes Station by Dr. Jordan; 10,000 studied by Dr. Vaughn, who in his studies, compared his cases with the 1918 outbreak; on Dr. Frost's investigations in Baltimore, and his studies here, all confirming the same fact, that in the recurring waves we had the same percentage of attack from those who had the disease before as from those who did not.

Dr. Symonds—Does it show that those who had an attack of influenza previously did not suffer with the same severity in the second attack, that it might be a relative immunity and not an absolute one?

Dr. Rosenau—All immunity is relative. The immunity in this disease, as far as we have been able to discover, is weak, wears off quite quickly, and our observations, moreover, indicate that there is very little immunity.

Dr. Toulmin—I should like to move a special vote of thanks to Dr. Rosenau for coming over here and giving us this most interesting address.

The motion was seconded and carried by a rising vote.

Dr. Knight—If we were asked to name a leading practitioner whose specialty interested us most, we

would think of the next speaker this afternoon. I have known him for a long time—since his second year in the medical school, when I began to teach auscultation and percussion, and he began to teach me! All of you know of Dr. Joslin—many of you know him personally. You know what an authority he is on this subject of Diabetes. You all know how perplexing and difficult and almost unanswerable a problem it is to some of us. It therefore gives me great pleasure to introduce to you Dr. Elliot P. Joslin, of Boston.

Dr. Joslin—Mr. President and Gentlemen: It is a great privilege to come here this afternoon, and I certainly appreciate it. I realize the power which you exercise, and that that power has always been for the good of the community and for the promotion of sound medicine. I have known your President for a long time. Our loss at Harvard has been your gain, but just think if he had stayed at Harvard what he might have done for us!

DIABETES AND LIFE INSURANCE

By Elliott P. Joslin, M.D., Boston

INTRODUCTION

The medical examiner and the doctor interested in diabetes have much in common. The work of each is largely of a statistical nature, and the success or failure achieved is largely based on the accuracy employed in the gathering of statistics and their interpretation. In many diseases skill in diagnosis is

required, but in diabetes faithfulness in the performance of routine examinations and faithfulness in the carrying out of routine treatment accomplish most and keep the patients alive the longest space of time. It is true that any day the discovery of an extract of the Islands of Langerhans may be made which will cure diabetes, and there is no question that the erratic but brilliant scientist will be the one who makes that discovery. Pending that date, improvement in treatment rests largely on the comparison of the relative amounts of various foods employed during the treatment with the end results. It is the duty of us all to encourage the brilliant investigator wherever he may be found, but it is likewise the duty of each one of us,—medical directors of insurance companies and practicing physicians,—to carry the statistical method to the limit, and fortunately this method has elements of fascination.

The debt which the public owes to insurance companies cannot be overestimated. The demonstration of your associations that the individual who takes the pains to be insured lives the longest should be continually emphasized. Fortunately it is true that in insurance and in medicine philanthropy is profitable.

I. OBSERVATIONS UPON CASES OF DIABETES FIRST DIAGNOSED BY EXAMINATION FOR LIFE INSURANCE

A. An increasing percentage of cases of diabetes now undergoing treatment has been discovered by life insurance companies. The first series of 1000 cases ending in 1916 showed 6 per cent. diagnosed in this manner; the second series of 1000 cases ending in 1920 9 per cent., and a third series of 300 cases showed 13 per cent. Altogether there are 190 cases of diabetes in my series discovered at examinations for life insurance. How helpful these diagnoses have been to the subjects concerned will later be seen. My interpretation of these figures is that insurance companies are to-day far more thorough in their selection of risks rather than that there has been any marked increase in the disease throughout the community. It is true

that in 1910 the rejections for one large insurance company on account of diabetes amounted to 5.3 per cent., and in 1915 to 7.3 per cent., but it is questionable whether there will be continual progress in this direction. There are already signs that the height of frequency of the disease has been reached.

TABLE I

Period	Number of Cases in Series	Sex		Discovered by Life Insurance		
		Male	Female	Total Per Cent.	Per Cent. of Males	Per Cent. of Females
1893–1916	1000	594	406	6	10	0
1916–1920	1000	578	422	9	15	1
1920–1921	300	159	141	13	22	2

B. The rôle of sex in diabetes and life insurance. The differentiation of cases of diabetes according to sex is particularly interesting. More men than women are treated for diabetes according to all authors, but more women than men die of the disease according to mortality statistics. In a collection of 7827 cases of diabetes, only 33 per cent. were females, yet of 11,775 cases of diabetes who died in the registration area of the United States in 1915, 54 per cent. were females. The preponderance of treated male diabetics compared with the preponderance of fatal female diabetics is probably explained by the greater frequency of urinary examinations of the males. It is gratifying that an increasing number of female diabetics are now being discovered as shown in the three series of cases just cited. In the first thousand the females were 41 per cent., in the second 42 per cent., but in the third series of 300 cases 47 per cent. Sex, therefore, plays little part in the frequency of diabetes. The discovery of the increasing number of cases in females is simply due to the more accurate medical methods recently employed.

Insurance companies have been responsible to a considerable extent for the large number of cases detected in males. Among the first thousand cases coming for treatment, 59 diabetics were discovered by life insurance companies and all were males. Thus, 10 per cent. of my male diabetics in this series were discovered by life insurance. In the second thousand 93 cases were discovered by the medical examiner. Of this number 88 were in the 578 males, making a percentage of 15 per cent., and 5 were found in the 422 cases of females, making a percentage of 1 per cent. In the third series of 300 cases there was a total of 38 discovered by life insurance, of which 35, representing 22 per cent., were males and 3, representing 2 per cent., were females. Thus the benefits of life insurance examinations are gradually accruing to females, and furthermore in the last five years life insurance has been responsible for the detection of twice the number of cases of diabetes as formerly.

C. Duration of life of diabetic cases of life insurance group. At the Massachusetts General Hospital from 1824 to 1898, inclusive, 68 per cent. of the fatal cases of diabetes died during the first year of the disease, and even between 1913 and 1917 48 per cent. did not outlive one year. The average length of life of all diabetics was longer. In 720 fatal cases of my own series ending in 1919 it amounted to 5.4 years. To-day it is a conservative statement that the average length of life of diabetic patients, including all cases from infancy to old age, is at least seven years. In contrast to these somber figures it is good news to record that the duration of life of 24 fatal cases, originally discovered on application for life insurance, was 12 years. The average expectation of life of these 24 cases at the onset of their disease was 27 years. It was, therefore, advantageous for insurance companies that these applicants were rejected, but do you wonder at my being grateful to insurance companies for early diagnosis of diabetes and the demonstration that as a result of early diagnosis the lives of adult diabetics have been doubled?

D. The causes of death of these 24 insurance cases in my series are not all known,—coma, cardiorenal complications, and

septic infections including gangrene, and pneumonia occurred about equally. Only one case is reported to have died of tuberculosis.

2. TESTS FOR DIABETES IN URINE, BLOOD, AND WITH THE DIET

A. Urine. 1. Routine chemical tests. The Benedict test for sugar in the urine appears to be the most satisfactory. The solution keeps indefinitely, the result is prompt and precise. The only objection to the Benedict test is its delicacy. Not everyone who shows a positive Benedict test for sugar has diabetes. It is a conservative test for insurance companies. If the patient is living upon a normal diet and the test is negative, diabetes is excluded.

Individuals whose urines react in a slightly positive manner to the Benedict test are the despair of insurance agents, a worry to medical examiners, and consumers of a physician's time. In general in the treatment of diabetes and its diagnosis by insurance companies it is safe to consider any patient to have diabetes mellitus and treat him as such, until the contrary is proven, who has sugar in the urine demonstrable by any of the common tests.

The Benedict test, however, does frequently give a reaction when there is comparatively little evidence that the patient has diabetes. Thus, among the urinary reports of the last 302 supposedly non-diabetic cases coming to my office, there were 52 who showed a positive test for sugar with Benedict's solution. In this group there were 23 who showed a positive reaction with Fehling's solution, and three showing a negative test. Quantitatively the sugar varied in 25 instances between 0.1 per cent. and 0.6 per cent. Subsequently the urines of practically all of these cases were tested again and evidence obtained that few of the patients were diabetic, but there was one case which was definitely diabetic. Six cases were obese and surely incipient diabetics, one was a case of renal glycosuria; six were cases showing disease of stomach, duodenum,

gall bladder or pancreas; 4 were of unusually nervous temperament; 4 of the patients had high blood pressure and the three remaining cases were associated with syphilis, cerebral disease or concentrated urine respectively. It is notable that though few of these cases were diabetic, the positive Benedict test appeared when there was other evidence of organic pathology. Although a positive Benedict test does not prove a patient has diabetes, it does necessitate the further examination of the urine and often of the blood as well.

2. The New Chemical Tests. New urinary tests for diabetes are beginning to come into vogue. Already Stanley Benedict has claimed that these tests may be as reliable or even more reliable as tests of diabetes than tests of sugar in the blood. The new chemical urinary tests are really tests for reducing bodies, undoubtedly carbohydrate, but the exact nature of these bodies is not definitely known. The principle upon which the Stanley Benedict and the Berglund tests are based is the quantitation of reducing substances after creatinin, uric acid, and similar bodies have been removed from the urine. Benedict frees the urine of these complicating substances by the use of mercuric nitrate, while the Berglund method makes use of Lloyd's alkaloidal reagent. All of us, medical insurance directors, and practitioners, should take cognizance of these new tests, because they may be of great advantage, not only in diagnosing the case of diabetes, but in discovering the potential diabetic. The obvious advantage of these tests is that they do not in the least annoy the patient. Conservatism would prevent their general application until more experience with them has been acquired.

B. Blood. Tests of the blood for diabetes are both less and more satisfactory than the urinary tests in disclosing the presence of the disease. They are less satisfactory because they are more complicated and the danger of the introduction of errors is considerable. Within a few weeks serious mistakes have come to my notice where the tests were performed in laboratories undoubtedly of good repute. Any clinician who critically examines reports of tests for the blood sugar in hun-

dreds of cases will confirm the above statement. The tests are in their infancy; they are not yet sufficiently simplified. Excellent technicians simultaneously examining the blood will obtain somewhat diverse results even if the same method is employed, and if different methods are followed the divergence is still greater. Fortunately there are several excellent tests: the Folin test, and the Benedict test with its various modifications, and in Europe Bangs' method is extensively employed. The Benedict test gives the highest results for blood sugar, next comes the Folin test, and the Bangs method gives the lowest results. As a rule a blood sugar is considered normal when the blood is taken before breakfast and the percentage of sugar is 0.10. Even after a meal or after a hundred grams of glucose or a gram and a half of glucose per kilogram body weight, the blood sugar should not rise above 0.15 per cent., and after a glucose tolerance test it should fall to normal or below normal at the end of two hours. Blood sugar curves above the normal range are usually satisfactory in the diagnosis of diabetes provided the presence of other diseases is excluded, such as Bright's disease, goitre, and perhaps cancer. Blood sugar tests, therefore, have their limitations; they are by no means pathognomonic. It is quite likely that they will become less rather than more reliable as increasing evidence is gathered regarding their interpretation.

C. Diet. Tests for diabetes with the diet are the most reliable of all tests. The individual who can eat ordinary meals and show a urine free from sugar after two successive normal meals is a safe risk for an insurance company. The question of course arises: What is a normal meal? By a normal meal is meant a meal which contains 100 to 150 grams of carbohydrate and about 25 grams of protein and 25 grams of fat. If there is reason to suspect diabetes in an individual a safe test for the presence of the disease would be the ingestion of two successive full meals like the above, but containing at least 125 grams carbohydrate. Such meals would consist of a moderate amount of meat or fish, a medium sized potato (20 grams carbohydrate), a portion of rice or macaroni (20 grams carbo-

hydrate), three slices of bread (60 grams carbohydrate), and a dessert of apple pie (30 grams carbohydrate) and ice cream (20 grams carbohydrate), with coffee and 2 lumps of sugar (10 grams carbohydrate). The urine should be collected during the subsequent two hours. It is necessary to give two meals in succession, because the modern diabetic patient is so well drilled in the diet that if he were malicious he might be able to so arrange his food beforehand that he could eat one meal containing even so much carbohydrate as that mentioned without sugar appearing in his urine. No diabetic, however, could take two such meals in succession without showing sugar. Should an exception be found to this statement, that individual would undoubtedly be a good insurance risk, because the diabetes would be so mild and he so skillful in controlling it that his expectation of life would probably be far higher than that of the ordinary person. This method of determining the presence or absence of diabetes is therefore recommended.

It is true that glucose tolerance tests are frequently employed to prove or disprove the presence of diabetes. They, too, are in their infancy, because they depend upon examinations of the blood for sugar. Recently a patient who had shown considerable amounts of sugar on many occasions took a glucose tolerance test and the result by itself would have excluded the presence of diabetes. Furthermore, conclusions as to glucose tolerance tests are complicated, because inferences cannot safely be based upon a single blood sugar examination. Three such examinations should be made: one before the patient takes the glucose, another in one half to three quarters of an hour, and a third at the end of two hours. If the subject, even though not a diabetic, has been upon a low diet for any considerable length of time, the blood sugar curve after the ingestion of 100 grams of glucose is not of a typical character, but higher and more prolonged, so that in such an instance he might be classified erroneously as a diabetic.

The rare case of renal glycosuria, even after a glucose tolerance test, may show normal blood sugar values. It is not proposed here to discuss the eligibility of cases of renal glycosuria

for life insurance. They represent an entirely different class of risks. As yet I am not convinced that cases of renal glycosuria are innocent.

Although glucose tolerance tests with examinations of blood sugar are not recommended as a routine, they are not so objectionable to-day as a few years ago, because the laity is becoming accustomed to such tests. The 4,000,000 of soldiers have all told their friends about the number of injections or tests which they received, and in consequence the public now are beginning to demand that they be studied as thoroughly as the soldiers.

3. THE PREVENTION OF DIABETES AND PREVENTION OF DIABETIC COMPLICATIONS

The active interest shown by insurance companies in measures looking toward the improvement of national health in general, but in particular toward the improvement of the health and the prevention of death of their policyholders, has been a subject of universal approval. How well these efforts have succeeded has been shown by one of the constituent companies represented in your association. The greater decline of the death rate for the policyholders than for the general population is evidence that these efforts have been successful. It is remarkable what you have accomplished for your clientèle in tuberculosis, organic diseases of the heart, Bright's disease, cancer, and contagious diseases. Already you have demonstrated to the financial branches of your companies that preventive medicine pays. There is no question but still greater opportunities lie before you, not only in the development of present lines of work, but in the exploitation of new lines. Preventive medicine in insurance companies offers an ideal field for work, because, first, the number of individuals to be considered is limited, and, second, end results are invariably known, an advantage which is not adequately appreciated. Although diabetes does not appear as frequently as the diseases above mentioned in your causes of death, one should not forget

that it does involve a million inhabitants in the United States.

A. The Prevention of Diabetes. The prevention of diabetes in your policyholders now free from the disease is worth while. Sensational measures must obviously be avoided. A guarantee that preventive measures will be successful can hardly be furnished, but there are possibilities for prevention which one must not overlook. Certain individuals in the community are susceptible to diabetes, and the factors which stamp an individual as most susceptible are:

1. Obesity. After the age of forty the disease is apparently 19 times as frequent in those of normal weight or above weight as it is in those who are 5 per cent. below weight, and after the age of fifty this disproportion is still greater. Fortunately in the prevention of obesity one is taking measures to prevent various other chronic diseases, such as disease of the circulatory and renal systems, so that advice disseminated by the various companies which you represent which would tend to prevent obesity in those above the age of thirty-five should yield good returns. It is imperative, however, in warning against obesity to be careful to state that the insurance tables show that under the age of thirty-five individuals who are overweight are those who have the best expectation of life. Recently in a college town in the West it was quite evident that the students were undernourished.

2. Heredity. Heredity has been shown by all writers to render individuals susceptible to diabetes. According to the accuracy of the data its presence is increasingly shown. In a series of 1187 cases heredity was demonstrated present in 21 per cent. With the knowledge that heredity is an important factor in the etiology of diabetes, the prevention of obesity in those thus predisposed should be attempted with especial care, and the promotion of good hygiene emphasized.

3. The Remaining Method. There is no question as to the benignity of diabetes discovered early compared with diabetes discovered late. The only way to secure the diabetic patient at an early stage is to hunt for him by increasing the frequency

of urinary examinations. The laity quite as much as physicians should be taught the importance of routine urinary tests, and even if such are made only as often as upon one's birthday, a great step in advance will be made. Laboratories and drug stores should be encouraged to make these examinations, and efforts should be exerted to simplify them. At present too much money is charged for the examinations of urine. In order to make the return appear the equivalent for the money expended, many laboratories issue a report on large sheets of paper and give details which are of no value. For the diagnosis of diabetes and Bright's disease in this connection, simple tests for sugar and albumin are all that are necessary, and if urinary tests of this character can be generally introduced and popularized the increasing number of tests performed will lessen the expense. Already some insurance companies have taken hold of this matter in a practical way by offering free yearly examinations to their policyholders. This is to be greatly commended. This custom should be more generally adopted, and advertised more widely. The insurance company who by a yearly examination discovers the advent of diabetes profits in so doing by prolonging the life of its policyholder. Should the urine be found normal a report to that effect to the client may lead to increased insurance. In other words, whatever the result, the insurance company gains. The practice of yearly examinations should also be taken up by general practitioners, and as with insurance companies it is advantageous for them also to offer free examinations of this type to their patients surely as often as once a year. Young doctors are the ones who will introduce this practice. The expense to the physician as to the insurance companies is negligible, the gain to reputation would be incalculable. Surely the family physician ought to be as interested in prolonging the life of his patient as the insurance company in prolonging the life of its policyholders. From the commercial standpoint alone the prolongation of life of a patient is fully as valuable to the doctor as that of the policyholder to the insurance company. The diabetic detected early is the diabetic longest treated. If the insurance companies do

not examine the urine of women, all the more reason that practitioners should especially examine the urine of women. Attention to the increased mortality of diabetes in women and the decreased number of cases treated should be spread broadcast. It is evident that there is more chance to-day in discovering new cases of diabetes by examining the urine of a thousand women than by examining the urine of a thousand men.

Why should not the insurance companies and doctors be as up to date as dentists? Dentists have their patients come at stated intervals for preventive work. They teach us both a lesson.

4. Promotion of Research in Diabetes. From the founding of the Massachusetts General Hospital in 1822 until 1898 for every hundred diabetics entering the institution 27 died within its walls, and even for the period from 1898 to 1914 the mortality remains practically the same: namely 28 per cent. So far as hospital mortality is a judge of advance in treatment no progress was made during these years. With the introduction of new methods of treatment in 1914 the mortality promptly decreased until for last year, 1920, the percentage mortality was 2 per cent. Since April, 1919, there have been under my observation at the New England Deaconess Hospital, the Corey Hill Hospital, the Brooks Hospital, and a diabetic boarding house 499 cases, and of this number there have been 11 deaths, a mortality of 2 per cent. Such gains in treatment, unequalled in any other chronic disease during the same space of time, have come about through research. It is quite possible that many of the cases entering hospitals since 1914 have been milder than those who formerly entered, but to make up for these, other cases in advanced stages living beyond the expectation of life of those of former days are now treated. The real basis for this great improvement is the discovery of the value of undernutrition. Undernutrition, though perhaps accidentally discovered by Guelpa, was only made available by Allen's painstaking work. Many other influences have contributed to the lessening of mortality, but undernutrition leads all and the insurance companies to-day owe a debt to Dr. Allen. The actual capital

saved insurance companies by this prolongation of life must be considerable and will continue to grow with each succeeding year. Hitherto the obligations of the medical profession to the insurance companies have been emphasized, but this is an instance where the insurance companies are indebted to physicians. Expenditures by insurance companies in the promotion of investigation of diabetic problems are surely as justifiable and would be as likely to lead to as good returns as expenditures by electrical corporations and chemical corporations in the maintenance of their scientific laboratories.

B. Prevention of Diabetic Complications. Diabetic patients seldom die of the disease diabetes, but rather of its complications.

1. First in importance is diabetic coma. Nine out of every ten diabetic patients who succumb to the disease in the first year following its onset die from this cause. Formerly fully two-thirds of all cases were carried off in this manner, but today the percentage of deaths from coma is considerably less and it probably is now responsible for about one-half of the deaths. Diabetic coma generally occurs in the neglected diabetic and as an accident, and like all accidents is to a large extent preventable. This complication, more than any other, causes a loss to insurance companies. It is to their interest to promote sound treatment to prevent it.

2. The second cause of death in diabetes which falls most heavily on the insured, particularly because of their age, is diabetic gangrene. The prevention of a wound to the feet, which usually is self-inflicted by the patient in caring for his toe-nails and his corns, though sometimes acquired by injuries received through tight shoes or nails in the shoe, should easily be accomplished. Insurance companies might with profit send to their policyholders, known to be diabetic, cautions along these lines, and with the advice to seek prompt treatment when the slightest abrasion of the skin occurs.

3. Other causes of death in diabetes are those which occur with advancing age, and are closely connected with the circulatory and renal systems. Pneumonia is not infrequent. In-

anition occasionally develops, but as yet has not reached a place of importance. There are more diabetics to-day who are in a stage of inanition than there were formerly, but this is almost entirely due to the fact that the counterpart of the patient with inanition who is alive to-day, a decade ago would have been dead. Inanition usually means that the life of a diabetic has been preserved beyond its former expectation.

4. The prevention of diabetic complications is of great importance in accident insurance. The sick man cannot be considered a better accident risk than a healthy man, and it is self-evident that wounds in an individual already diseased have a poorer tendency to heal than in an individual in perfect health. It is good business for accident insurance companies to demand yearly urinary examinations of all policyholders over 50 years of age. It is probably safe to say that any operation known to surgery can be performed successfully upon the diabetic patient, but one cannot claim that it is as safe to operate upon a diabetic patient as it would be upon an individual in perfect health. It is for the insurance companies, therefore, and particularly accident insurance companies to discover whether any of their policyholders are afflicted with disease, and in particular with diabetes. It would seem to a layman that accident insurance companies would save enough in the end should they go to the expense of urinary examinations of all their members.

Dr. Knight—If we did not have another address in this whole meeting, I should say that the meeting would have been worth while. We will now hear from Dr. Muhlberg, who will open the discussion of Dr. Joslin's paper:

Dr. William Muhlberg—Mr. President and Gentlemen: I would not presume to discuss this able paper in a critical way; this paper which has been presented by Dr. Joslin. The essayist is a recognized authority on diabetes, and his numerous contributions—both clinical and research—have greatly advanced our knowledge of the subject.

His work is of peculiar interest to this Association in that, in a most authoritative and convincing manner, he has called attention to the relation of "overweight" and diabetes. Almost epigrammatically he states in one of his articles "with an excess of fats, diabetes begins, and from the excess of fats, diabetics die." Succinctly, this expresses the modern trend of thought on the pathology of this most interesting disease.

His observations are abundantly substantiated by our medico-actuarial statistics. In Volume II, *Medico-Actuarial Investigation*, page 34, appear these figures:
Cause of Death—Diabetes.

Ratio of 10,000 Exposed to Risk.

	Overweight 50 lbs. or More	Standard	25 lbs. Underweight
Age at entry.....15–29	1.5	0.6	0.7
Age at entry.....30–44	5.9	1.2	0.5
Age at entry.....45 & over	13.6	2.8	0.6

It will be noted that at ages 15 to 29, overweights compared with lightweights, die of diabetes in the ratio of about 2 to 1. At ages 30 to 44, 12 to 1; at ages 45 and over 23 to 1.

I have collected data on 1000 consecutive policyholders in our Company, who showed in response to our free health test, one fourth per cent. or more of sugar in their urine. These figures are presented in a diagram similar to that appearing in Dr. Joslin's paper, entitled: "The Prevention of Diabetes Mellitus"—except that I have used pounds overweight and pounds underweight, instead of giving percentages. The data are somewhat disappointing in that they do not demonstrate any marked relationship between glycosuria and the weight-factor. But it must be remembered that most of the cases are probably alimentary glycosuria, and quite a few of the lightweights were suffering from underweight—the result of the malnutrition which often accompanies the last stage of the disease.

Twenty-four of these policyholders have died. Thirteen were lightweights and ten of these succumbed to diabetes. And of the nine heavyweights only two were diabetic. This is the table:

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Age Years	No. of Cases	Below Standard Weight in Pounds					Normal	Above Standard Weight in Pounds								Percentage of Each Decade Below Normal
		41 50	31 40	21 30	11 20	6 10	−5 +5	6 10	11 20	21 30	31 40	41 50	51 60	61 70	71 80	
15–20	3	0	0	1	0	0	1	0	0	1	0	0	0	0	0	33%
21–30	53	0	0	4	12	10	14	4	4	3	1	0	1	0	0	49%
31–40	154	7	14	27	30	8	18	11	10	12	6	6	3	1	1	56%
41–50	301	9	15	35	49	15	49	20	34	30	28	8	5	1	3	40%
51–60	317	1	16	19	38	22	51	30	46	36	30	11	6	4	7	30%
61–70	141	4	2	18	21	9	27	11	20	14	7	4	2	0	2	38%
71 plus	31	0	4	3	5	5	5	2	2	2	2	1	0	0	0	55%
1 to 71 plus	1000	21	51	107	155	69	165	78	116	98	74	30	17	6	13	

40.3%	16.5%	43.2%
13 Deaths	2 Deaths	9 Deaths
Ex. Goiter.............. 1 Chr. Myocarditis........ 1 Diabetes............... 10 Tuberculosis........... 1	Duodenal Ulcer......... 1 Diabetes............... 1	Angina Pectoris............ 2 Drowned.................. 1 Chr. Bright's.............. 1 Per. Anemia............... 1 Carbuncle................. 1 Diabetes.................. 2 Acute Card. Dilatation...... 1

It might be interesting to speculate how overweight causes or predisposes to diabetes. I believe the experimental evidence, that it is a disease of the pancreas, is almost indisputable. It is not, however, quite so clear why the pancreas gradually loses its function of helping the tissues in the normal processes of glycolysis. Very likely chemical injury resulting from years of faulty dietetic habits is the most important element; just as the heart breaks down under prolonged physical strain, so may the pancreas weaken from repeated chemical or dietetic insults.

Allen has shown that in partially de-pancreatized dogs the remnant of pancreas will become inefficient and diabetes will develop if the animal is fed excessive amounts of carbohydrates over a long period.

In a great majority of cases obesity results from overeating, and this vice in some persons breaks down the kidneys; in others, it probably acts similarly on the pancreas. Bright's disease as a cause of death at all ages is about five times as frequent among heavyweights as it is among lightweights. May not the overweight in itself play also a part in a vicious circle?

Normally, the body disposes of most of the ingested carbohydrates in one of two ways—either it burns them up, or it converts part of them into glycogen and fats. May not an overweight whose fat depots are already overcrowded have more difficulty in disposing of his carbohydrates?

Clinically, it sometimes happens that a diet rich in fat increases the glycosuria. Since the body cannot convert fats into sugar, it is not impossible that this anomaly may be explicable on a similar hypothesis.

Extreme hyperlipemia is quite as pathognomonic of advanced diabetes as is extreme hyperglycemia; but mild cases have apparently a relatively normal fat metabolism. Might obesity be looked upon as one of the earliest symptoms of diabetes?—an effort on the part of the body to dispose of the carbohydrates through converting them into fats, rather than to permit them to be wasted, by excretion through the kidneys.

It is true that young persons do not apparently so readily store up fats. Could not this in part account for the more rapid course at younger ages, since their inability readily to convert carbohydrates into fats might partially block one of the avenues for disposal of surplus sugars that are not directly oxidized?

Dr. Joslin, I believe, does not incline to the view that chronic nerve influences—worry, anxiety, business responsibility, bad temper, etc.,—are an etiological factor in diabetes. But, physiologically, emotional conditions do mobilize the carbohydrates and cause glycosuria. Would it be unreasonable to conjecture that, given a patient with a pancreas below par, on account of racial or hereditary weakness, repeated nervous hyperglycemia might further break down the pancreatic mechanism.

Quite a few insurance risks, whose clinical histories I have followed over a series of years, have impressed me as being diabetics of this type.

The answer to this question is of some importance to us, because on the assumption that nervous or alimentary glycosuria is not a very serious impairment, insurance companies have been accepting risks with a single past history of sugar in the urine. The medico-actuarial mortality of these cases was 103 per cent. (?)—but the deaths from diabetes was six times the normal. Dr. Rogers's studies indicate that the impairment is much more serious than the figures given by the medico-actuarial tables, where sugar is found oftener than once.

Our company attempts to select among them through the glucose test, and to date our experience has been favorable.

There is one phase of Dr. Joslin's work that is of the greatest importance, not only to insurance companies but to the community at large. Diabetes (and the same is true for cardiovascular conditions), tuberculosis, cancer, and other chronic conditions must be diagnosed early, otherwise, the results of treatment are disappointing. Unfortunately, this phase of medicine has been badly neglected.

Dr. Joslin has taken a foremost stand, not only in calling

attention to this matter, but he has gone further in attempting to outline prophylactic measures by remedying overweight. Contributions of this sort are of the greatest aid to us in our conservation work—so much so, that I am going to ask Dr. Joslin's consent to present the material in his article entitled: "Prevention of Diabetes Mellitus" to our policyholders who are overweight.

Dr. Knight—Dr. Exton will continue the discussion.

Dr. Exton—Mr. President and Gentlemen—Insurance medical men sometimes express the feeling that they are not particularly interested in the treatment of disease. May I say that as regards Diabetes at any rate this is not quite true. We are certainly interested in the treatment of Diabetes, because, as Dr. Joslin has pointed out, the risks we take to-day, who may have or who may develop Diabetes can be expected to live longer than similar risks which we put on our books in former years.

It is not unknown, but is, perhaps, not generally appreciated, that Dr. Joslin gave the diabetics of the world about ten years' advantage of the Allen treatment. It is usually conceded that the Allen treatment would have lain somewhat in abeyance for about ten years had not Dr. Joslin explained its value and endorsed it. This was a great gift and one in which we share.

To one who is familiar with diabetics and the course of the disease, probably the most impressive evidence of the efficiency of the modern treatment by under-nutrition are the results seen in connection with surgical complications which have always been so much dreaded and which were usually expected to end in death. To-day it is pleasant and surprising to be able to expect that many of the cases when they are properly handled will recover from gangrene and other surgical complications.

It has occurred to me that in addition to examining our policyholders' urine once a year in our various health tests, that

we might with frequent advantage go a step further. Outside of the larger medical centers the modern treatment is not always given as accurately as it might be and it is worth while considering how our diabetic policyholders may be led to have the benefit of well-managed modern treatment. In a few instances we have found it possible to do this with remarkably gratifying results. Likewise when we take a risk which is somewhat questionable, one that comes to us with previous records of glycosuria or one who may show sugar in the urine on examination, it occurs to me that we might be a little personal with such individuals and try to prevail on them to send us a specimen of urine from time to time in the effort to keep track of them with the object of having them receive the benefit of early treatment, if they should happen to need it. Some such plan should prove helpful, because, as Dr. Joslin has pointed out, the early treatment of the disease means so very much to patients and hence to our companies too.

From the practical standpoint of the medical man in the office who has to handle the glycosuria cases, the Diabetic problem is often difficult if one attempts to treat each case on its merits as I think we should. It seems to me there is no question about our aiming to reject those who are diabetic and accept those who are not, although I feel that the time is coming when the so-called pre-diabetic and some of the incipient mild diabetics will be regarded as insurable; in fact we have already insured some of them.

The absolute decision as to the existence of the disease is not always easy because of the extremes to which some people will go either to get or to avoid getting life insurance. Some twelve years ago I was called one evening to see a man who had a temperature of 107 following a chill, and a very virulent urinary infection as a result of the injection of supposedly normal urine into his bladder in order that he could get down town and pass it for one of the insurance companies. At the other extreme, and I think Dr. Rogers will recall the case, a physician who wanted insurance came to see us and tried to explain away previous records of sugar in his urine by telling

us that he had injected some glucose solution into his bladder so as to have the agent leave him alone. These are, of course, extreme cases, but they illustrate the difficulties with which we have to contend.

When a doubtful glycosuria is under consideration I have a little habit of asking myself two questions: First, When is a Diabetic not a Diabetic? The answer is—When he wants Life Insurance. The other question is—When is a Glycosuric not a Diabetic? This is a more difficult proposition and we try in several ways to get at the right answer. In the first place if there is only a slight reduction (we use Benedict's test) we determine definitely that the reducing substance in question is or is not diabetic sugar. In the summer time, especially, we find quite a few reactions which are not due to glucose. In women, particularly those who are lactating we are often able to spot lactose. During the sugar famine we found quite a number of reducing substances which proved not to be glucose, and from time to time various quinones are found which simulate marked glucose reductions and which are present only because of some kind of medicine which is taken usually for cathartic purposes. If it is ascertained that we are definitely not dealing with glucose we are inclined not to pay attention to reductions unless there are other doubtful features involved in the case. Even in definite glucose cases we frequently investigate the possibility of the glycosuria being transitory and caused by some recent digestive upset or mental strain. To do this we use the Tolerance test, giving something like three tablespoons of cane sugar (granulated) with some cake or luncheon. One must certainly agree with Dr. Joslin that the Tolerance test is not all that it might be and that one can not invariably count on its results, but up to the present we do not know of any better way of getting at the truth with regard to doubtful cases.

In this connection the latest technique of Benedict and Osterberg for quantitating minute amounts of sugar in the urine bears on a subject we have often discussed, namely, the amount of normal sugar in the urine. In his most recent paper

in the September *Journal of Biological Chemistry*, Professor Benedict does not state, as some who have spoken here on previous occasions, quoted him. In fact, he makes no statement at all as to what is the normal amount of sugar a normal urine contains. He does, however, give a table of "The Comparative Amount of Sugar in Normal Urine," and of these supposedly normal urines the highest is given as 0.21 per cent. of sugar. It is currently told that this particular case has since developed into a frank diabetic and I call attention to this not only because it leaves the question as to how far one may go in the interpretation of the delicacy of our copper tests still somewhat open, but particularly because some insurance medical men appear to have very settled opinions with regard to the insignificance of finding less than one-quarter of one per cent. of sugar in a chance specimen of urine. Personally, I have never trusted the accuracy of the methods we have hitherto had at our disposal for estimating these minute amounts of sugar and for that reason look upon this new Benedict-Osterberg technique as a distinct advance. I am hoping that with it we may now be in a position to establish something like a dead line in connection with the Tolerance test. The method is not difficult to carry out and with the Colorimetric technique which has been developed in our Laboratory which eliminates all of the troubles in connection with the preparation of standards for color comparison the test becomes quite simple.

I think that Dr. Joslin is quite right with regard to what he has said about blood work. Our experience bears him out. We have tried to get at the truth of some large exceptional cases by blood sugar determinations made by reputable men here and there, and have satisfied ourselves that if we are to do blood work at all it must be done in our own laboratories. It is the same lesson over again that we learned from Home Office urinalysis, and you can not depend on a man here and a man there making blood tests for you; even with the same technique and standards the results will not check. We have been doing some experimental work and have been getting others to

experiment for us in an effort to find a way of preserving the blood so as to get it into the Home Office Laboratory in a fit condition. Miss Denis has found that one drop of formalin will preserve five cubic centimeters of blood for four days and we have been able to establish the correctness of this, but we do not feel that we can trust the examiners to get us exact amounts of blood and add exact proportions of anything to them. The ferments which are normally in the blood split up and destroy the sugar as soon as the blood is withdrawn, and we have been hoping to find a method which will prevent this and have had some success by preventing the access of air to the blood. If the sugar in the blood can be preserved intact for some days there would seem to be no inherent difficulty in getting results which are accurate enough for our purposes with a drop or two of the finger blood.

May I thank Dr. Joslin for the privilege of hearing him again?

Dr. Knight—Dr. Balch will continue the discussion of Dr. Joslin's paper.

I have heard Dr. Joslin's paper with much interest, and think his suggestions of great value to us in insurance work. We have border line or suspicious cases in which the blood sugar determinations would be of importance. Up to the year 1918, blood sugar determination offered the only positive method of deciding whether or not an individual was a safe risk.

BLOOD SUGAR. UPPER LIMIT

I am particularly struck by the comparatively low limit which Dr. Joslin gives for blood sugar in normal individuals. The high normal is usually about 0.15% after a carbohydrate meal or administration of glucose, but the percentage may go higher. King Goto and Nobuzo Kuno of the Tokyo Imperial University, published recently (*Arch. Int. Med.*, 27, 1921) under the title "Studies in the Renal Threshold for Glucose," the results of an investigation which has a bearing on this ques-

tion. Blood sugar was determined in the cases of 53 normal individuals, after fasting, and after taking 100 grams of glucose. Tests for sugar in the urine were made by the Benedict qualitative method.

In 20 cases, no sugar appeared in the urine after glucose.

Fasting plasma sugar....................	average 0.092%
After glucose, maximum plasma sugar between 0.128 and 0.185%...............	average 0.142%

In 28 cases, traces of sugar appeared in the urine after glucose.

Fasting plasma sugar....................	average 0.093%
After glucose, maximum plasma sugar between 0.128 and 0.196%...............	average 0.160%

In 5 cases, in which return to the normal was delayed, following administration of glucose, the maximum plasma sugar reached about 0.2%.

It would appear from these studies that we may have a plasma sugar reaching 0.2% after administration of glucose test meals, in individuals who are quite normal.

Perhaps the chief objection to blood sugar determination in insurance work, is the difficulty in obtaining specimens. Applicants for insurance may decline to furnish the desired specimen, and Field Examiners may be lacking in the special skill necessary to obtain blood from a vein. Certainly the urine offers an easier path to the desired information than does the blood, provided that the information secured by this means is conclusive.

SUGAR IN NORMAL URINE

We have known definitely of the presence of sugar in normal urine for about 25 years. Earlier studies indicated the presence of glucose, as well as of other sugars, in small quantity, but it remained for Baisch to demonstrate in 1895 (Baisch, K., *Z. physiol. Chem.*, 1895, xx., 248) and Breul in 1898 (Breul, L.,

Arch. exp. Path. u. Pharm., 1898, xl., 1), the fact that reducing sugars are constantly present in normal urine. Since this period, many others have reported the habitual presence of sugar in normal urine, but accurate methods for determination of these small quantities have not been available. We may omit mention of certain fairly accurate methods proposed during the past ten years, and refer to the one published by Benedict and Osterberg in 1918 (*J. Biol. Chem.*, vol. xxxiv., p. 195). The process is accurate to within a few thousandths per cent. It depends upon precipitation of substances other than sugar, by mercuric nitrate in alkaline solution, and estimation of sugar colorimetrically after treatment with picric acid. Recently Benedict and Osterberg have simplified the process (*J. Biol. Chem.*, Sept., 1921, xlviii., No. 1) and the procedure can now be carried out much more rapidly. In the last named process, the specimen is shaken with bone charcoal, treated with picric acid and acetone in presence of alkali, and sugar determined colorimetrically.

Using these methods, Myers and others have shown that an increase in the quantity of urinary carbohydrate promptly follows the ingestion of carbohydrate food. In the normal subject, the increase is usually small in amount, and there may be no increased percentage, the quantity being shown only when volume is taken into consideration. With a slightly lowered carbohydrate tolerance the actual percentage of urinary carbohydrate is increased to a marked extent. Using these methods, it appears that we are able to determine the assimilation limit from the urinary findings as accurately as from blood analysis.

We are perhaps not interested in the sugar content of normal urine, but we are greatly interested in the question "what is normal urine? We have long known that the more commonly used sugar reagents, such as Fehling's and Haines, are sometimes highly inaccurate. Urine contains substances other than sugar which give reactions like sugar with these reagents, and urine also contains substances which interfere with precipitation of the cuprous oxide which results from the action of sugar

on such reagents. Further, we may have in urine, sugars other than dextrose, and these include pentoses, isomaltose, and lactose. Phenylhydrazine is more reliable than the copper reagents, and the quantity of crystalline osazone formed gives one a fair idea of the amount of sugar present. Also, the character of the crystals is a fair index of the kind of sugar present.

In the Benedict-Osterberg methods, total sugar is determined in a portion of the specimen. A second portion is treated with yeast, and after fermentable carbohydrate has been removed, the non-fermentable portion is determined. The difference between the two figures represents fermentable sugar. Using this method, we have frequently found that specimens of urine which had given well marked reactions with Fehling's solution, reported as "large trace," contained a normal quantity of sugar, a few hundredths per cent. Again we have found specimens which gave no reaction with Fehling's solution, to contain as much as 0.118% sugar. Specimens reported "slight decolorization" may contain 0.166% sugar. As a rule, Fehling's reagent will give some reaction with quantities of sugar over 0.06%.

UNUSUAL QUANTITIES OF CARBOHYDRATE IN THE URINE OF NORMAL INDIVIDUALS

We must consider a few hundredths per cent. of fermentable and of non-fermentable sugar as being normal. What is to be thought of the occasional specimen which shows quantities of sugar in the neighborhood of 0.25 to 0.5% and perhaps more? We have tested out several of these cases with the aid of 100 grams glucose, administered in 10 to 12 ounces of water, in the fasting stomach. Occasionally we find a large portion of the glucose returned to the urine, obviously a diabetic condition. Often the urine, following the glucose test, is normal, or, again, the quantity of sugar may be slightly above the normal. These cases are not diabetic, and they are safe insurance risks. But why the unusual quantity of carbohydrate in the urine at the earlier examination? Benedict and Osterberg have shown, in

their work, that a normal individual may excrete as much as 0.6% sugar in the urine of a single voiding, the carbohydrate diet being high. Another subject excreted 0.22% sugar. We may quote as follows: "The percentage of sugar in the urine on a high carbohydrate diet varies between 0.078 and 0.608. It is perhaps needless to point out that urines containing 0.6 and 0.4 per cent. of sugar give very marked qualitative tests for sugar with the Benedict copper solution. Had these particular samples of urine happened to be examined by a clinician the conclusion would probably have been drawn that the subject was a diabetic. Almost certainly life insurance would have been refused as a result of such an examination, yet the subject has apparently no diabetic tendency. For the clinician to find such a tendency would simply require a combination of circumstances by which the urine sample should be collected within two hours after a breakfast unusually rich in carbohydrate. Probably not infrequently individuals are rejected for life insurance owing to such a combination of circumstances. An accurate quantitative determination of the 24-hour sample on the ordinary diet would correct the error."

Further, Benedict and Osterberg show that glucose, taken with a meal, gives rise to much more marked sugar excretion than does the same quantity of glucose taken in the fasting stomach. Cane sugar does not have this effect to the same extent. Twenty grams glucose, added to a weighed breakfast, resulted in excretion of 0.138% of sugar at one time and 0.55% at another. It would seem that grapes or other fruit, rich in this form of sugar, taken by a normal individual with a meal, may result in the excretion of reducing sugar in quantity.

Dr. Joslin has mentioned the fact that he found sugar, in small or large quantity, in the urine of 52 out of 300 patients in whom there had been no reason to suspect the presence of diabetes. This is in accord with the results of a study made in our laboratory about three years ago. We tested the urine of about fifteen individuals over a period of several days, making tests several times each day. Sooner or later we were able to obtain small sugar reactions in the urine of practically all.

8

These small sugar reactions are certainly not to be considered as evidence of presence of diabetes; on the other hand, we regard them as being perfectly normal. This is not a plea for you to consider cases showing from one half to three quarters per cent. sugar as being normal, although perfectly normal individuals occasionally show these quantities of sugar. My special point is that small traces of sugar, traces for which some Insurance Companies have rejected or postponed acceptance of applications, are worthy of further study. In the majority of cases, these quantities of sugar are of no importance at all.

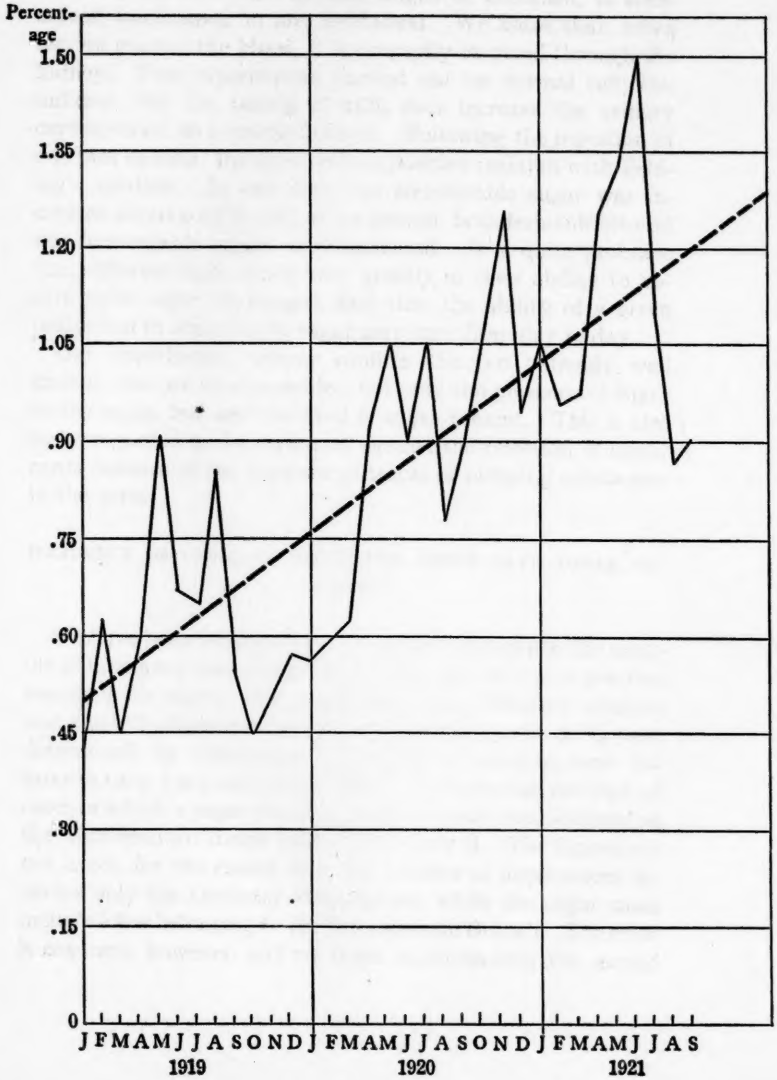
IDENTIFICATION OF URINARY CARBOHYDRATE

Dr. Joslin has not mentioned the identification of sugar in the urine, but we assume that he has some plan for separation of the different varieties of carbohydrate. We have studied carbohydrate excretion along these lines in our laboratory. One of the sugars known to occur in the urine is isomaltose. This sugar is utilized in the human body less readily than are the other common sugars. In view of the prevalent use of extracts of malt, in various forms, and the fact that we so frequently find osazone crystals in our phenylhydrazine test, that appear to be derived from maltose and isomaltose, we consider these sugars to be of special importance in insurance work. We have given a normal subject, 187 grams of one of the commercial extracts of malt. Following this administration, the urine reduced Fehling's solution in an atypical manner. Total sugar was found to be 0.104% and 0.118% at two and three hours. The same quantities of sugar were found before the malt extract was taken, although the urine was then negative to Fehling's. After malt extract, the fermentable sugar had disappeared, and the entire quantity present was non-fermentable. The quantity of malt extract taken in this experiment was large, and the work should be repeated, using varying quantities in several subjects. The results are suggestive, however.

It is a matter of common knowledge that milk sugar is fre-

CHART I

Percentage of Cases Giving Sugar Reactions in Territories D, E, F, G, and H for 1919, 1920 and 1921, by Months



quently found in the urine of women during the period of lactation. Personally, we have long been of the opinion that such an easily diffused carbohydrate might be absorbed, to some extent, unchanged, in any individual. We know that, when lactose reaches the blood, it is promptly excreted through the kidney. Two experiments, carried out on normal subjects, indicate that the taking of milk does increase the urinary carbohydrate to a marked extent. Following the ingestion of one pint of milk, the urine gave a positive reaction with Fehling's solution. In one case, the fermentable sugar was increased about 0.05%, and in the second, both fermentable and non-fermentable sugars were increased. It is quite probable that different individuals vary greatly in their ability to absorb milk sugar unchanged, and that the ability of a given individual to absorb milk sugar may vary from day to day.

Our experiments simply confirm the fact, already well known, that we must consider, not only the presence of sugar in the urine, but also the kind of sugar present. This is also to be regarded as an argument against the rejection of applicants because of the presence of traces of reducing substances in the urine.

INCIDENCE OF CASES IN WHICH THE URINE GAVE SUGAR REACTIONS

We have been interested in the marked increase in the number of insurance cases received, in which the urine gave positive reactions for sugar. For qualitative tests, Fehling's solution and phenylhydrazine were used, and quantities over 0.2% were determined by polariscope. Methods of analysis were the same in 1919, 1920, and 1921. Chart 1 indicates the per cent. of cases in which a sugar reaction, large or small, was obtained at the Metropolitan Home Office Laboratory B. The figures are not exact, for the reason that the number of applications includes only the Ordinary Department, while the sugar cases include a few belonging to the Intermediate Branch. The error is constant, however, and we think it affects only the second

decimal place. The upward trend is apparent. In chart 2 we have plotted a curve showing percentage of cases in which the quantity of sugar was above 0.75%. Here the upward trend is very slight, practically zero. Chart 3 shows percentage of cases in which less than 0.75% of sugar was found. The trend is practically the same as in chart 1. We are indebted to the Metropolitan Statistical Bureau for the trend lines which appear on the charts.

AN ASSOCIATION OF LABORATORY WORKERS

There are many officers in charge of the laboratories of the Insurance Companies who are not members of this Association. We know that methods of analysis vary greatly, and we know that the methods of reporting various items quantitatively for code, L, M, and N, differ to such an extent that these letters have little meaning. It would seem that an association of laboratory workers would serve a useful purpose in the way of standardization of methods. Mutual suggestions and criticism would be of much assistance, and the reports and recommendations of such an association, to the Medical Directors Association, should be of great value.

Dr. Archibald—There is one question which I should like to ask. We are all very much impressed over the question of sugar in the urine. I remember about two years ago, in the spring of the year, a case of two brothers who were applying for insurance; both young healthy adults of average weight, good build and excellent family history. Both these applicants on examination on the same morning showed a very considerable amount of sugar. It seemed rather peculiar that these two brothers should show a very positive reaction, and I questioned them concerning their diet, and learned that both had eaten griddle-cakes with maple-syrup for breakfast.

The question which I should like to raise is—has the particular type of carbohydrate which a person happens to ingest any marked effect on the reduction reaction in the urine?

Another point—someone referred to the Islands of Langer-

CHART II

Percentage of Cases Giving Sugar Reactions of .75% and over in Territories D, E, F, G, and H for 1919, 1920 and 1921, by Months

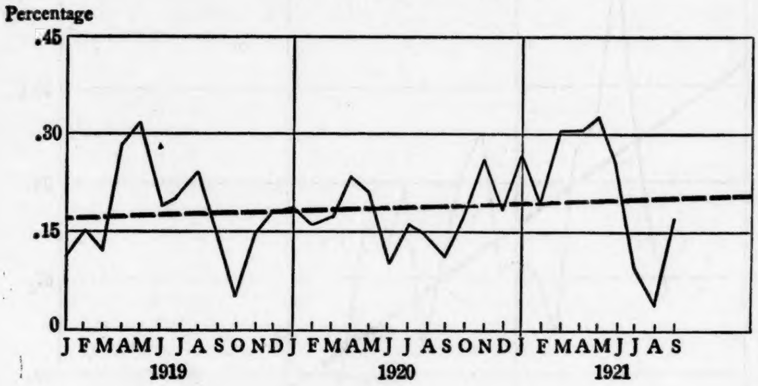


CHART III

Percentage of Cases Giving Sugar Reactions under .75% in Territories D, E, F, G, and H for 1919, 1920 and 1921, by Months

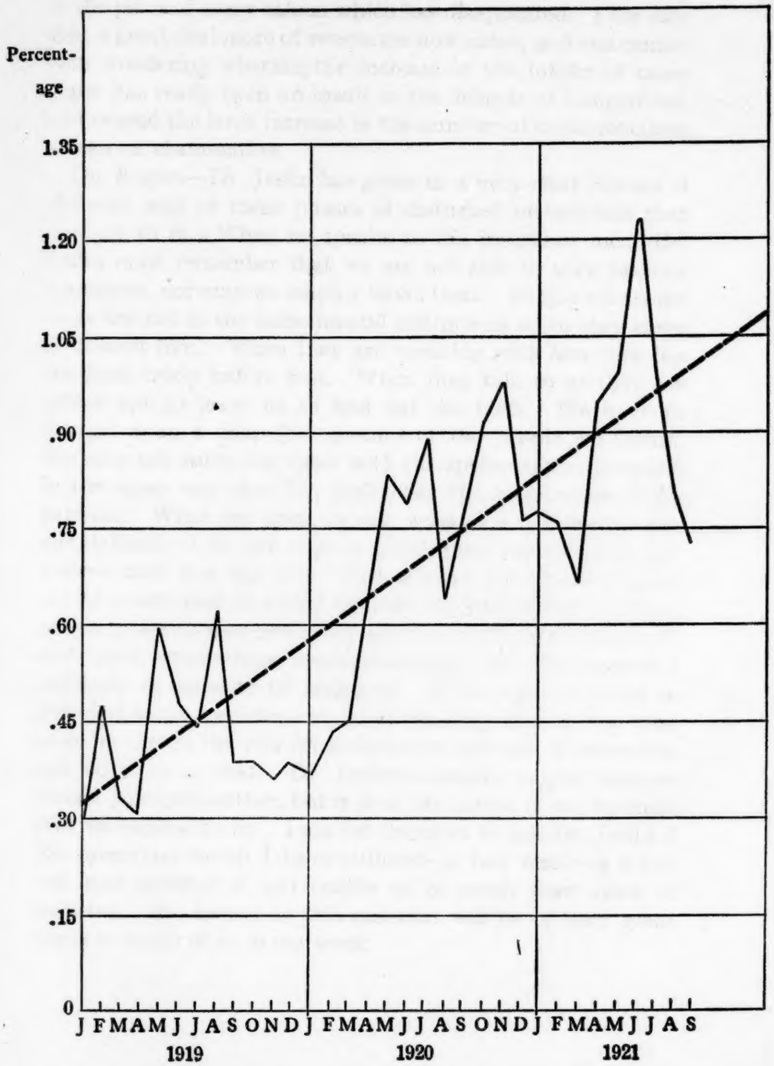
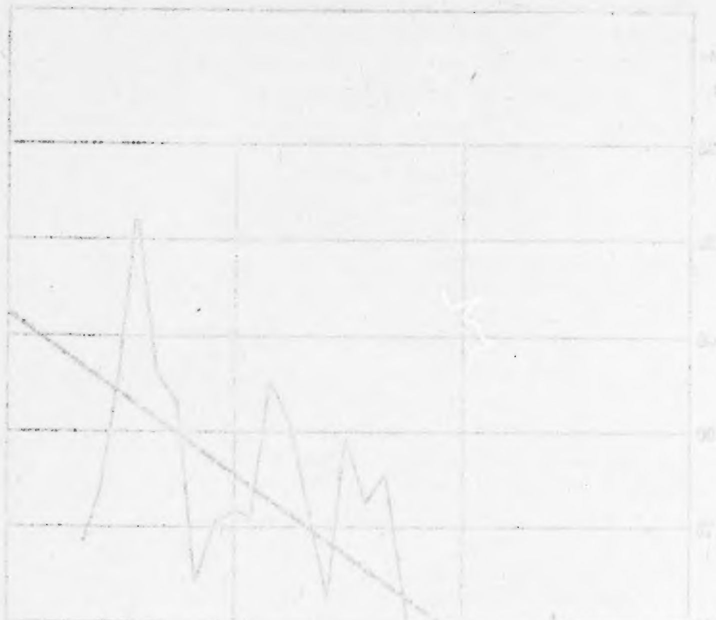


CHART III

Percentage of Cases Giving Sugar Reactions under 75% in Treatments D, E, F, G, and H for 1919, 1920 and 1921, by months.



hans being insulted. Dr. Balch noted the increase in the number of sugar reactions in 1919–1920–1921. In Ontario, where prohibition exists, three candy stores have sprung up in the place of every saloon which has disappeared. I am sure that a great deal more of sweets are now eaten, and one cannot help wondering whether the increase in the intake of cane-sugar has really been an insult to the Islands of Langerhans, and caused the large increase in the number of sugar reactions shown on examination.

Dr. Rogers—Dr. Joslin has given us a very clear picture of diabetes and of those phases of disturbed metabolism that lead up to it. When he speaks to life insurance men, Dr. Joslin must remember that we are not able to take 24-hour specimens, nor may we employ blood tests. People who come to us are not in the same mental attitude as when they come to consult him. When they are speaking with him they lay the facts freely before him. When they talk to us they are rather apt to leave us to find out the facts. We have to depend upon a snap-shot picture of the people we insure. We may not study our cases with the applicant's coöperation in the same way that Dr. Joslin has the coöperation of his patients. What we need in our work is a fair test of the metabolism,—I do not mean a glucose test because I do not believe that is a fair one. Recently we have been making use of a test meal of mixed proteins, carbohydrates and fats, such a meal as a man ordinarily eats; and four hours afterwards and again twenty-four hours afterwards, we have secured a specimen of urine to be examined. If no sugar is found in either of these specimens we have recommended to the company to accept the risk for a moderate amount of insurance, and let it go at that. Dr. Joslin's remarks suggest that we should go much further, but it is in the nature of our business that we cannot do so. I am led therefore to ask Dr. Joslin if the procedure which I have outlined—a test meal—is a fair test and whether it will enable us to catch most cases of diabetes. His answer to this question will be of very great value to many of us in our work.

Dr. Symonds—I would like to ask one question. The Benedict Test is a reduction test. Now in our office we feel that when we get a positive reaction with one of the reduction tests, we then check it always with the fermentation test. There are times when the fermentation test does not respond when there is a reduction test. In one week, nearly one-tenth of the cases I saw showing a reduction did not show anything at all by fermentation, laid carefully in a thermostat with a control specimen. Is it safe to take those cases as non-glycosuric which show such a reaction?

Dr. Joslin—The discussion has interested me very much. Taking up the points in reverse order—as regards the Benedict and fermentation tests, I have come here to learn from you, rather than to give information, because you all have had so much experience with these tests. One must be sure that he has the right sort of yeast and that it does not contain carbohydrate. One must always run a control and one must always consider the possibility that by the time one gets to the point of beginning fermentation the sugar has not already had an opportunity to disappear. All these are possibilities. In the main the fermentation test is very reliable.

When sugar shows in the urine, it is conservative for that man to be studied carefully, before he is taken for life insurance. That brings me to Dr. Rogers' question about any simple and quick decision. The diabetic of to-day is skillful, and a great many of my cases of diabetes could have eaten the luncheon we ate to-day and shown no sugar at all. The luncheon which was offered us was certainly what one would call a square meal. It happened, however, to be not very rich in carbohydrate but a meal with meat, two or three slices of bread, potato, a piece of apple pie, ice cream, and two or three lumps of sugar in the coffee would catch practically everybody, but it would not catch all of them. It will catch almost all. Two such meals, eaten in succession, should be sufficient to indicate any diabetic of sugar to an insurance company, provided the urine remains sugar free.

Dr. Willard—What would be the best time to get the urine?

Dr. Joslin—Get the urine soon. Not have the man go out and come back. Have him sit there, and pass his urine in four hours. I would keep him right under my eye. In fact, I would rather keep him two hours, than have him go out and come back in four.

As to the question of an excess of carbohydrate at the present time leading to an increase of frequency of sugar being found in the urine. That may be possible, but it is not yet proven, and furthermore even if proven, it would not necessarily mean diabetes. I have tried to get ideas on the subject, but have not succeeded.

That leads of course to the alcohol question, because the reduction in calories taken in alcohol may or may not have been counterbalanced by an increase in calories taken as carbohydrate.

The newer methods of disclosing reducing bodies in the urine by the Benedict and Osterberg test, and when the Benedict test is negative, are still new methods. Let us wait five years, and note what happens to one thousand individuals studied and followed during this period. I am conservative with my patients. I have seen far too many patients who have told me that they had a trace of sugar and disregarded it five years earlier, but were frankly diabetic when they consulted me.

The normal blood sugars after a meal. A blood sugar of 0.15 per cent. is high enough when the sugar in the whole blood is examined. I think Dr. Balch referred to plasma sugars, which are a little higher. Incidentally this proves my point about the practical value of test meals with urinary examinations as compared with blood sugar tests. We are not quite certain of the blood sugar test. Do you ask for a plasma sugar to be done, or for a total blood sugar to be done? There is a difference. I think it is safer to follow Dr. Rogers's plan, and give the client a good square meal, and test the urine, rather than to depend upon blood.

It would not be surprising if almost every individual would show sugar in the urine if single specimens were persistently

examined. I know of no facts. Work along this line is being done by Professor Folin and Dr. Berglund.

It would appear better to me to have insurance companies encourage the improvement of treatment of diabetics by all practitioners rather than to send a few diabetic cases to a few specialists. Recently Dr. Allen spoke to the point when he said practitioners should treat diabetes whenever they feel that if a case developed in their own family they would be competent to care for it.

That made the members of the State Medical Society who heard the remark think a little bit. Dr. Allen is all right so far as he goes, but I believe diabetes is not the problem of one man or any group of men, but of all practitioners. There are a million diabetics. All practitioners should treat them. The treatment of diabetes should be popularized.

Overweight. Dr. Muhlberg's tables are most valuable. His data and mine are not so far apart as they seem, and for this reason. Ask anyone for his weight and he may reply 175. But what I have always done is to say—"Do you mean that you really never weighed over 175?" Then the reply will come, "Once the weight was 180." I then say, "Was that stripped or dressed?" and often I can raise the weight ten pounds more. A woman often states "She never weighed more than 150 pounds," but when I take the weight it proves to be 160, 175, and sometimes 180. So we must be careful. It is the man underweight who has difficulty in developing diabetes. We must take into account the difficulties Dr. Muhlberg had in getting the weights, because he had to depend often on the reported weights instead of the actual weights, and didn't have a chance to actually weigh the applicants, so I really think our tables come out not so far apart. My opportunities for securing accurate data upon weight are better than those of insurance companies who deal with reported weights gathered from many sources.

Dr. Knight—The next paper to be discussed is the one presented by Dr. Rogers and Mr. Hunter—

"Ratings for the Principal Impairments"—and as galley proof was sent out in advance, this paper will not be read here, but we will proceed immediately with the discussion.

RATINGS FOR THE PRINCIPAL IMPAIRMENTS

BY ARTHUR HUNTER and DR. OSCAR H. ROGERS

The great majority of medical impairments occur comparatively infrequently in routine insurance practice. A dozen of these impairments make up nearly 90 per cent. of the substandard cases handled in the course of our daily work. Of these, some have already been discussed in other papers, and the remainder of them are considered in this paper with the object of answering numerous inquiries as to our treatment of the principal medical impairments.

The impairments which present themselves to Medical Directors in the course of their routine work will vary greatly in the different companies, will be distinctly fewer in number in the companies which limit themselves to standard lives, and correspondingly more numerous in companies which do a substandard business. Aside from occupations, the principal impairments met with in the routine of handling our company's substandard business, arranged in the order of their frequency, are as follows:

Heart murmurs,
Overweight,
Albumin in the urine (with or without casts),
Consumptive family history (usually with underweight),
Irregular, intermittent, or rapid pulse,
High blood pressure,
Unsatisfactory habits as to alcohol—past or present,
Syphilis, history of,
Gastric and duodenal ulcers, history of,
Gallstones or biliary colic, history of,

Gravel or renal colic, history of,
Sugar in the urine,
Lung trouble, history of,
Applicant not robust,
Goiter,
Inflammatory rheumatism, history of,
Underweight,
Pleurisy, recent history of,
Asthma,
Middle ear disease.

The first three of them cover nearly one-half of the total, and none of those listed after the first ten occurs in more than 1 per cent. of all substandard cases.

It should be noted that the foregoing list applies to substandard risks. The order of their frequency would have been materially changed had standard risks with minor impairments such as asthma and middle ear disease been included, as some of these occur quite frequently among applicants accepted by our company at the regular rate of premium.

Our medical ratings are expressed in the same manner as that employed in the Medico-Actuarial Mortality Investigation, excepting that, instead of giving the total mortality due to an impairment, only the amount of the excess mortality above the normal is given—thus +50 per cent. means a mortality of 150 per cent., +100 per cent. a mortality of 200 per cent., and so on. The ratings therefore apply to practically any system of selecting risks for insurance. The letters "R. N. A." mean "Risk not Acceptable," and no ratings are given.

These ratings should not be looked upon as final but only as expressing the best in information now available on the subjects to which they apply. We have had occasion in the past to make changes in them, and expect to do so in the future as new facts are brought out to render such changes necessary. Ratings which may correctly express present-day experience may be found in the future to be either too lenient or too severe.

HEART MURMURS

Our ratings on heart murmurs are based upon the same statistics as those employed in our paper entitled "Heart Murmurs—Their Influence on Mortality," supplemented by additional information obtained since that time. We employ them with such variations in individual cases as the circumstances in each case seem to justify. A person with a heart

HEART MURMURS

	Very Best. No Hypertrophy	Average No Hyp.	With Slight Hyp.	With Moderate Hyp.	With Considerable Hyp.
Mitral regurgitation	+ 60	+ 75	+100	+125	R.N.A.
Mitral obstruction. .	+200	R.N.A.	R.N.A.	R.N.A.	"
Aortic regurgitation	+200	"	"	"	"
Aortic obstruction. .	+ 75	+100	+125	+150	"

	Up to age 35	Age 36 to 45	Age 46 and over
Functional.....	+10	+30	+50

HISTORY OF HEART MURMUR

Functional—Disregard if two examiners report no murmur.

Mitral regurgitation...........................	+20 to +40
Aortic obstruction.............................	+35 to +50
Aortic regurgitation...........................	+50 up
Mitral obstruction.............................	+50 up
Double murmurs................................	+50 up

lesion, whose occupation involves considerable physical exertion or mental effort, should obviously receive a rating higher than one whose life is free from business anxiety or physical strain. These adjustments of the ratings applicable to individual cases the selector must make in such fashion as to secure in the group as a whole the mortality desired by his

company. He must not modify *favorably* some of the cases unless he modifies *unfavorably* others of them. The ratings are true only for average cases and not for the most or the least favorable.

These considerations enter to a still greater extent with regard to those risks where there is a *history* of a heart murmur, in which, as will be noticed from an inspection of the schedules, a good deal of leeway is permitted in the standard ratings. By a "history of a heart murmur," we mean: (*a*) a record by our own company; (*b*) a finding by another company; (*c*) a statement by the applicant himself; (*d*) a report from a physician. If two competent examiners, knowing that a heart murmur has been found in the past, fail to find it a year or more after the original record, the history is disregarded, unless there are several records. Of heart murmurs that are found today and are not found next month by equally competent examiners we have no statistical evidence upon which to base an opinion. Such cases are rated conservatively on consultation between members of our Medical Board.

When there is a history of acute articular rheumatism or where the pulse is rapid, irregular, or intermittent, an addition is made to the sum of the ratings for the two impairments. It may be recalled that the experience of the New York Life was unfavorable in these cases.

OVERWEIGHT

Our ratings for overweight were given in our paper on the "Numerical Method of Determining the Value of Risks for Insurance," published in 1919. As to these overweight risks it may be well to repeat that the ratings apply to average risks and not necessarily to any given risk. Any considerable departure from the schedule rating should rest, however, on clear proof that the individual case under consideration differs decidedly from the average of its class. Overweight risks are usually presented in most favorable terms. Whatever the real

facts may be the overweight is almost invariably a well-marked family characteristic. Rarely are they described as flabby or obese. On the contrary, they are said to be remarkably vigorous and active persons, large-boned and heavily muscled. Yet it is with just such material that the life companies were dealing when they experienced the mortalities disclosed in the Medico-Actuarial Investigation.

ALBUMINURIA

In a paper on "The Effect of Glycosuria and of Albuminuria on Mortality," published in 1917, we gave the experience of our company with albuminuria. Since that time, we have had an opportunity to study additional material from another source.

The significance of albuminuria depends upon several factors, the amount of it found and whether it is constant, intermittent, or occasional. A company which limits its acceptance of cases of albuminuria to risks in which only faint traces are found occasionally, would naturally have a better mortality than that experienced by a company which freely accepts on substandard plans cases showing a moderate amount of albumin constantly. It is essential therefore in comparing the experience of different companies to know the types of albuminuria dealt with in each of them. These shade insensibly from those in which it is only rarely found in traces to those in which it is found constantly in large amounts. We distinguish between them as follows:

Accidental—if found once in several tests.

Intermittent—if found in two out of three or four tests.

Constant—if found in two out of two, or three out of three or four tests.

In determining the amount of albuminuria, the standards recommended by the Association of Life Insurance Medical Directors are followed.

Our schedule of ratings is as follows:

ALBUMINURIA

Accidental 0

INTERMITTENT

	Ages		
	Under 30	30–45	Over 45
Faint trace	0	+ 10	+ 10
Trace	0	+ 10	+ 20
Moderate	+ 10	+ 20	+ 30
Large amount	+ 50 up	+ 75 up	+100 up

CONSTANT

Faint trace	+ 10	+ 20	+ 25
Trace	+ 25	+ 35	+ 50
Moderate	+ 40	+ 50	+ 75
Large amount	+100 up	+125 up	+100 up

With respect to a history of albuminuria, our practice is to impose an additional rating of from 10 to 30 points, depending upon the conditions, the age of the applicant, and whether the history is recent or remote. We usually disregard a history of more than two years standing, provided the record has been entirely cleared in the meantime and several intervening examinations have shown no albumin.

CONSUMPTION IN THE FAMILY HISTORY

We have no reason to change in any respect the ratings for this impairment, as published in October, 1919, in a paper on "Numerical Method of Determining the Value of Risks for Insurance."

RAPID PULSE

The principal source of our information on this subject is the Medico-Actuarial Mortality Investigation, supplemented by our own experience.

The following ratings are employed:

RAPID PULSE

Rate of Pulse		Additional Mortality Expected
90–100	....	+50 to + 75
100–110	....	+75 to +125

A rapid pulse appears to be equally significant at all ages and foreshadows disease of the heart or blood vessels, or indicates tubercular disease.

SLOW PULSE

As was shown in the Specialized Mortality Investigation, a slow pulse is an advantage rather than a disadvantage. Where the pulse rate is from 55 to 65, we usually allow a credit of 10 points; if the pulse rate is below 55, it is a cause for suspicion, perhaps of fatty heart, and an attempt is made to investigate the condition of the heart muscle.

IRREGULAR OR INTERMITTENT PULSE

Our experience among lives with a persistently irregular or intermittent pulse shows a distinctly high mortality. The material available was not sufficiently extensive to enable us to differentiate between the intermittent and the irregular pulse, so that we still treat them as if they were of the same significance. The element of age appears to be of importance, as well as the degree of irregularity or of intermittence. Our schedule of rates is as follows:

IRREGULAR OR INTERMITTENT

	Ages at Entry			
	15–30.	31–45.	46–55.	56–up.
5 or less per minute............	+20	+40	+ 60	+ 80
Over 5 per minute..............	+30	+55	+ 80	+100
Irregular and intermittent.......	+40	+75	+100	+100

Disappear on exercise P.M. +10
40 & 50 +25
50 +50
40 & 50 +50
50 +50
40 +25

It will be recalled that the results of the Medico-Actuarial Investigation were favorable in these conditions, but the Committee very properly pointed out that the low mortality was probably due to the inclusion in the group of many cases in which the condition was temporary or caused by the excitement of the examination. Our ratings apply to cases which, on the whole, have been sufficiently studied to indicate that the condition is not temporary.

HIGH BLOOD PRESSURE

The Joint Committee of the Actuarial Society and the Medical Directors' Association recently investigated the question whether there was available in the records of the larger companies material sufficient for an investigation into the effect of blood pressure on longevity. It was found that a considerable volume of reliable material could be obtained on systolic blood pressure, but that the period of its exposure was quite short. Moreover the practice of taking the diastolic pressure has only become general within the last few years. It was therefore decided to delay an investigation into the effect of blood pressure until the experience had more fully ripened. The experience of our own company is necessarily limited and especially so when divided according to age and to degrees of departure from the average systolic and diastolic pressures. The testimony of rejected lives presented from time to time to the Medical Directors' Association by Dr. Fisher and the opinions of a number of clinical experts have also been called

upon to add to our knowledge. The whole subject is still in need of a thorough investigation and it is hoped that in a few years a coöperative study may be made of it to the lasting benefit not only of life insurance but of the science of medicine as well. Meanwhile, in our own practice, we have assumed that the *average* blood pressure is probably the *normal* blood pressure; that a departure of as much as 15 per cent. either above or below the average systolic pressure may be within physiological limits, and that, outside of those limits, we probably come into the field of the pathological, at any rate on the high blood pressure side of the average.

The following table gives the average blood pressure at quinquennial ages and the probable limits beyond which the systolic pressure may be looked upon as pathological:

MEN

Age at Entry	*Assumed Normal Limits*		
	Low	Average	High
20..........	100 mm.	120 mm.	137 mm.
25..........	102	122	138
30..........	103	123	140
35..........	104	124	142
40..........	106	126	144
45..........	108	128	146
50..........	110	130	148
55..........	112	132	151
60..........	115	135	154

The systolic blood pressure of women is slightly lower than that of men at the younger ages, but is about the same as that of men at the older ages.

A diastolic pressure is considered normal if it is from 25 mm. to 50 mm. below the systolic.

The schedule of ratings in use by us, which is necessarily a tentative one, is as follows:

SYSTOLIC BLOOD PRESSURE

Age at Entry	Record in Millimeters									
	135	140	145	150	155	160	165	170	175	180
	Mortality Ratings									
20	+10	+20	+30	+45	+65	+90	+115	R.N.A.	R.N.A.	R.N.A.
25	+ 5	+15	+25	+35	+50	+75	+100	"	"	"
30	0	+10	+20	+30	+45	+65	+ 90	+125	"	"
35	....	+ 5	+15	+25	+40	+60	+ 80	+110	"	"
40	....	....	+10	+20	+35	+50	+ 70	+100	"	"
45	....	....	+ 5	+15	+30	+45	+ 65	+ 90	+130	"
50	....	....	....	+10	+25	+40	+ 60	+ 80	+115	"
55	....	....	....	+ 5	+20	+35	+ 55	+ 70	+100	"
60	....	....	....	+ 5	+15	+30	+ 50	+ 65	+ 90	+125

It will be noticed in the foregoing table that the company does not look with favor upon blood pressure near the upper limits of the "normal" and employs a slight rating to express that disfavor. It will also be seen that it does not accept risks in which the extra mortality is above 130 per cent., i.e., a mortality of 230 per cent., while in certain other impairments risks are accepted whose valuation is considerably higher. Our reason for making this distinction is that we do not feel sufficiently confident of our standards to warrant us in advising the company to assume risks on these lives at high ratings. This is true also with respect to large amounts of insurance. Indeed it is our practice to accept risks with unusual care and for only small amounts of insurance in impairments regarding which the underlying data are scanty and with freedom if the statistical basis of our valuations is considerable.

LOW BLOOD PRESSURE

The only experience that has come to our notice on this subject is that of the Northwestern Mutual, which indicates that,

in cases accepted by that company as standard risks, the mortality has been satisfactory among persons with a low systolic pressure. Our practice has been to add a very slight rating if the *systolic* blood pressure is 10 mm. below the low limit in our table of normal limits and from +20 to +30 in cases in which the blood pressure is more than 10 mm. and less than 20 mm. below the low limit. Where the blood pressure is more than 20 mm. below the "low" line, a substantial rating is applied or the case is declined. In all cases of markedly low blood pressure, an effort is made to ascertain the cause—whether abuse of tobacco, use of drugs, a tubercular tendency, lack of vitality, or the like.

HABITS IN THE USE OF ALCOHOL

The applicants rated up by us for a record of past or present abuse of alcohol represent 5 per cent. only of all persons treated as underaverage risks, excluding those in occupations involving hazard. Nevertheless, they cause us far more trouble than many large classes, as there is no one subject about which the opinions of medical directors differ so greatly as about the treatment of persons whose past or present habits are questionable. Any attempt to classify them is likely to be more or less unsuccessful. Our standards are based on the results of the Medico-Actuarial Mortality Investigation, together with an extensive experience of our own. Great care is necessary in the acceptance of these risks as the anti-selection is likely to be severe. The applicants know their own habits best, and are therefore better able than the insurance company to judge whether the policy offered is to their own advantage or not.

Our standards, which follow, show that the Medical Board is allowed considerable latitude in rating these cases:

Steady Free User

(*a*) Steady, free user, never intoxicated........	+50 to +100
(*b*) History of, within 2 years................	+40 to + 80
(*c*) History of, 3 to 5 years...................	+30 to + 60
(*d*) History of, 6 to 10 years..................	+20 to + 40
(*e*) History of, over 10 years..................	+10 to + 30

OCCASIONAL EXCESS, STILL CONTINUING

(a)	Occasional excess for day or evening only, not oftener than six times a year........................	+50 to +100
(b)	More frequent, say once a month..................	+75 to +150
(c)	More frequently, say once a week.................	R.N.A.
(d)	Occasional excess lasting for two or more days but not oftener than three times yearly..................	+150 up
(e)	More than above..................................	R.N.A.

OCCASIONAL EXCESS, HISTORY OF

	Within 2 Yrs.	3 to 5 Yrs.	6 to 10 Yrs.	Over 10 Yrs.
(a)	+ 40 to + 80	+ 30 to + 60	+20 to + 40	+10 to + 20
(b)	+ 60 to +120	+ 45 to + 90	+30 to + 60	+15 to + 30
(c)	+100 to +200	+ 75 to +150	+50 to +100	+25 to + 50
(d)	+100 to +200	+ 75 to +150	+50 to +100	+25 to + 50
(e)	R.N.A.	+100 to +200	+75 to +150	+25 to +100

The lower of the two ratings in each class applies to persons who are now total abstainers and the higher to those who still use alcoholic beverages moderately.

A person who has taken a "cure" for alcoholic habits is treated the same as if he had not taken the cure but had given a history of occasional excesses or of steady free use, as the case may be.

SYPHILIS

The amount of evidence now available of the influence of this disease upon mortality is considerable. The experience of the combined American and Canadian companies (M.A.M.I.) and of several American companies published separately show an extra mortality ranging from 21 per cent. to 117 per cent. European experiences show an average extra mortality of fully 60 per cent. When the fact is recalled that, in the selection practiced by them in the past, the life companies have, as a rule, excluded the severer forms of the disease as well as those which have been quite inadequately treated, we are warranted in concluding that the experiences quoted above express the differences due to either greater or less care in treatment, or to

varying degrees of severity of the infection. It is unfortunate that there is at present available no conclusive direct evidence of the effect of treatment. The testimony of the M.A.M.I. is conflicting on the subject. Our own experience, based upon a careful selection with respect to both the severity of the disease and the thoroughness of the treatment, has been distinctly more favorable than that of the M.A.M.I. It has not been sufficiently favorable, however, to warrant our company in accepting as standard risks any of these cases however careful the treatment or mild the symptoms.

Our standards are as follows:

Cured	Thoroughly Treated	Not Thoroughly Treated
Initial lesion only..........	+30	+ 50
Slight secondaries.........	+40	+ 75
Marked saturation.........	+50	+100
Tertiary symptoms........	R.N.A.	R.N.A.

It will be noticed that in the heading of the schedule we make use of the word "cured." We do not mean by "cured" that the disease has been eradicated, for we doubt whether such a result is ever achieved with certainty. The record of locomotor ataxia, general paresis, and of vascular degenerations following syphilis is strongly opposed to that view. We mean simply that the applicant is free from all symptoms. By "thorough treatment" is meant adequate supervision for at least two years, including mixed treatment, and freedom from symptoms for at least one year before discontinuance of treatment.

GASTRIC AND DUODENAL ULCERS

The standards employed by our company in the valuation of these risks are largely based upon the testimony furnished by the Mayo Clinic, modified by our own experience. The report of that Clinic was published in 1919 in the Proceedings of the Medical Directors' Association. We have not dared to follow the testimony of the Mayo Clinic altogether, for the reason

that we have yet to learn whether the surgical treatment of these conditions is as efficient elsewhere as it is in that institution. Besides, in dealing with cases of this kind there should always be a margin of safety.

It will be noticed in both gastric and duodenal ulcers that the extra hazard is assumed to be a temporary one which disappears altogether after a certain number of years. This may not be entirely true, but the evidence so far at hand indicates that the hazard after a limited number of years is small if not altogether negligible. We doubt, however, whether cases of gastric ulcer may safely be accepted for large amounts for several years beyond the period covered by our ratings. We believe that there is probably an extra hazard for some time thereafter. As the element of age does not seem to be a factor, the extra mortality is expressed in terms of the number of extra deaths per 1000 for a period of years.

GASTRIC ULCER WITH OR WITHOUT OPERATION

Years Since Attack	
1st	R.N.A.
2d	R.N.A.
3d	80 extra deaths per thousand.
4th	45 extra deaths per thousand.
5th	30 extra deaths per thousand.
6th	15 extra deaths per thousand.

DUODENAL ULCER

Years Since Attack	Non-Operated
1st	R.N.A.
2d	40 extra deaths per thousand.
3d	28 extra deaths per thousand.
4th	15 extra deaths per thousand.
5th	10 extra deaths per thousand.
6th	5 extra deaths per thousand.

Years Since Operation	Operated
1st	R.N.A.
2d	10 extra deaths per thousand.
Thereafter	0 extra deaths per thousand.

It will be noted that in the case of gastric ulcer the same rating is applied whether the risk has been operated upon or not while in the case of duodenal ulcer without operation a much higher mortality is anticipated than in cases successfully operated upon. We should like to say frankly that this distinction does not rest upon any insurance experience to which we have had access, but rather upon the observation and opinions of surgeons who have had a wide experience in these cases.

In the practical application of these ratings an extra premium is charged for a short period and as these premiums are without loading, neither commissions nor dividends are paid upon them.

GALLSTONES, BILIARY OR HEPATIC COLIC

The results of the Medico-Actuarial Mortality Investigation of those who had one attack of gallstones (hepatic colic) or bilious colic showed practically the same mortality, about 128 per cent., irrespective of the time elapsed since the attack. The experience of the New York Life is in line with this experience, but is, on the whole, somewhat more favorable. Our treatment is the same whether the disease is described as gallstones, biliary colic, or hepatic colic, and is as follows:

	Within 1 Yr.	1–2 Yrs.	3–5 Yrs.	After 5 Yrs.
1 attack...........	R.N.A.	+ 50	+ 30	+20
2 attacks..........	"	+ 75	+ 50	+30
Repeated attacks...	"	+150	+100	+50

Drainage of the gall bladder is treated the same as one attack; but if the gall bladder has been extirpated, one-half of the foregoing rating is applied.

RENAL COLIC, GRAVEL OR CALCULUS

We have experienced difficulty in deciding upon a schedule of ratings for renal colic, because there have been marked

differences in the results of various investigations of the subject. Thus:

Medico-Actuarial................	106 per cent. of M.-A. Select.
Penn Mutual..................	115 per cent. of M.-A. Select.
Equitable Life................	94 per cent. of M.-A. Select.
Mutual Life (approximate).....	110 per cent. of M.-A. Select.

When these statistics are studied according to number of years elapsed since the attack, the results are irregular and confusing. The experience of the New York Life, to be made known when more material has been accumulated, shows a higher mortality than any so far published. We therefore use the following ratings, which are slightly lower on the whole than our own experience and somewhat higher than the aggregate of other published investigations:

RENAL COLIC, GRAVEL OR CALCULUS

	Within 1 Yr.	1–2 Yrs.	3–5 Yrs.	After 5 Yrs.
1 attack...........	+ 50	+30	+20	+10
2 attacks..........	+ 75	+50	+30	+15
Repeated attacks...	+100 up	+75	+50	+25

GLYCOSURIA

The basis of the ratings of the New York Life appears in the paper published in the *Transactions of the Actuarial Society*, October, 1917. While a great deal is heard about glycosuria, it is found much less often than albuminuria. Hardly one-sixth as many substandard policies are issued by our company to applicants with sugar as to those with albumin in the urine. Probably one of the reasons glycosuria plays a prominent part in our work is that it seems to occur more frequently among well-fed persons who apply for large amounts of insurance than among applicants for small policies.

In glycosuria it is important to differentiate between those

who are and those who are not receiving dietetic treatment. To determine this, it is now our practice in suspected cases, where the amount involved warrants it, to ask the applicant to eat a test meal. This meal, which is taken in the presence of the medical examiner, includes a fair proportion of bread, starch, and sugar. A specimen of urine is passed four hours afterwards, and another specimen twenty-four hours after the meal. If sugar is found in excess in either of these we look upon it as evidence that the case is under dietetic treatment and is uninsurable. On the whole, the company's experience with this simple procedure has been satisfactory, although occasionally an applicant is observed who passes the test and later is found to have sugar. Apparently, in these cases, the chemistry of the body is broken down with regard to some single article of food which happened not to be included in the test meal.

The following is our practice, which allows considerable leeway to the medical director in his rating of the risk:

Intermittent	—If clearly without diet or treatment.......	+ 50 to +100
	If with diet or treatment................	R.N.A.
Persistent	—If clearly without diet or treatment, but less than 1 per cent. sugar in urine.........	+100 to +150
	If with diet, treatment, symptoms, or with more than 1 per cent. sugar present in urine..............................	R.N.A.

Where there is a history of glycosuria but no trace at the present time, our treatment depends upon the time elapsed and the dietetic restrictions exercised. We disregard one finding by our own company, or one record in the past, provided repeated examinations over a reasonable period show that the previous findings were probably accidental.

A trace of acetone leads to further study. If it persists or occurs in quantity we decline to insure the risk.

OTHER IMPAIRMENTS

We have not dealt with several of the impairments which are mentioned at the beginning of this paper, nor with several

others, such as neurasthenia, appendicitis, asthma, gout, or malarial fever. Some of these impairments are not of much importance if the attacks were mild or occurred some time ago. At a later date we may be able to discuss them.

CONCLUSION

In conclusion we may repeat that, although the ratings presented by us have proved safe on the whole in the conduct of our substandard business, we expect to make changes in them in the future as new facts are brought out. We should like also to add the caution that, while they have proved entirely safe for our company, they may need modification if used by companies which follow different methods of selection or different business practices. They should not be looked upon as absolute ratios of extra mortality, but, rather, as reasonable guides in the selection of risks for insurance.

DISCUSSION OF RATINGS FOR THE PRINCIPAL MEDICAL IMPAIRMENTS

By Mr. Robert Henderson,

Second Vice-President and Actuary

and

Dr. Thomas H. Rockwell

Medical Director of the Equitable Life Assurance Society

The thanks of the insurance world are again due to Messrs. Hunter and Rogers for their frank submission of the extra ratings which their company has adopted to meet the extra mortality expected in certain impairments. These impairments when encountered in companies not doing a substandard business, should result in the rejection of the risk, for if accepted without a proper surcharge on some so-called "modified plan," they will result in the end in extra losses, which ultimately are borne by the policyholders who are properly standard risks.

Two years ago they presented their Build Table, which is the primary basis on which the risk is graded, and they explained that their table is arranged according to height and weight at different ages. At the same meeting the Equitable Build Table was explained, which adopts as standard the column of average weights for height given in the M.-A. experience for ages 35 to 39. Using this standard as a basis, the various percentages of over and under weight are calculated, the result being a very practical and easy working formula, lending itself readily to the numerical system of rating adopted by this Society. From it will be seen the percentage of over and under weights, and in the table immediately below, the percentage of mortality expected according to age and deviation from the standard. As a calculation of the basic mortality is the first step employed in the estimation of the risk, it is the custom of the Equitable to adhere very closely to the table, and only in unusual instances to permit the so-called element of personal selection or observation to enter. For, as will be seen, the table itself is sufficiently elastic to permit of debits and credits as they may accordingly appear in the individual risk.

We desire to add to the author's warning regarding the consideration of overweights that a claim for consideration on account of a favorable distribution of weight is a very frequent feature of such cases. Appeals for consideration on that ground should be to a certain extent discounted, as we should expect the girth of the abdomen to be somewhat less than that of the expanded chest, even in heavy weight cases. It will be noted that our ratings for distribution of build practically assume that the average cases will show an expanded chest at least one inch larger than abdominal measurement.

Heart Murmurs:

In a previous paper by Messrs. Hunter and Rogers, the great importance of the degree of cardiac hypertrophy, combined with organic murmurs, was well brought out. Our experience has been a very similar one, and in order that there might be a uniformity of rating on the part of the individual Medical

Director, we set about to devise a practical working basis. The first thing was to obtain a representation of a normal thorax. Our search through standard works on anatomy of the chest failed to locate just the sort of outline we desired, so we enlisted the co-operation of Dr. L. Gregory Cole, who took a number of X-rays of apparently well-formed and perfectly normal chests. At the same time fluoroscopic examinations were made to locate the normal position of the apex. The result was finally obtained in the chart which we present to you. Referring to the authors' table, you will observe that the rating, for instance, for a mitral regurgitant murmur, will vary greatly, according to the degree of described hypertrophy from plus 60 for the very best with no hypertrophy, to an absolute declination if considerable hypertrophy is present.

In order to simplify our work we made a device which is here presented. It has apparently been productive not only of evenness of rating on the part of the individual Medical Director, but the hope of beneficial results in our ultimate selection.

Albuminuria—With or Without Casts:

The ratings given by the authors for albuminuria agree very closely with those used by our Company if we remember that they apply to albuminuria without casts and that an additional rating is to be imposed if casts are present. Our practice is to consider these two impairments as specially additive, the rating for albuminuria and casts being the sum of the separate ratings. Our rating for hyaline casts alone is 10 points for each cast in one-half ounce of urine, using the average number found in the specimens examined. If casts are constantly present an additional rating of plus 50 is added, or if the number of casts exceeds 10, the case is declined. The rating for granular or other casts is obtained by multiplying that for hyaline by 1½.

This rule is based largely upon the experience of this Society, and in order that it may be useful to other Companies, it is necessary that a complete understanding be had as to the very

exact method we employ in determining the number and kind of casts present. A portion of the specimen is poured into a half ounce bottle, containing a preservative, which after years of use, has been proved most satisfactory, and which will undoubtedly preserve casts and other constituents of the urine for months. This half ounce is centrifuged for three minutes at a speed of 2000 revolutions per minute. A pipet removes all the sediment, which is placed on a large slide and examined with a low power lens, each cast being counted. For differential purposes a high power lens is used. If there is a history of casts, or if we find them in the first specimen, we call for two more samples; the result of the three combined are considered and the rating given in the table accorded. For companies using a different method than ours, we cannot advise the ratings we have adopted, but we find the method a simple one, and in our hands it has given good results so far.

Blood Pressure:

Up to now our greatest task has been to endeavor to obtain from our examiners a proper care in their technique, and to this end we have sent out numerous circulars and bulletins, explaining just how we would like to have the observations taken. The "stock" letter we distribute has helped greatly, and as both the systolic and diastolic is called for in every case, we feel we have been more than repaid by these circulars and letters, for we notice a very marked improvement in the accuracy of the observation.

Alcohol:

A very interesting point comes up in connection with the treatment of cases presenting a history of excessive use of alcohol. It may be supposed that the effect of the prohibition amendment would be to reduce the hazard in such cases. There may be in the long run a certain effect of this kind, but we cannot say that it has yet been demonstrated. Some statistics regarding our claim experience of the past few years may be of

interest in this connection. We have examined our records for deaths due to acute and chronic alcoholism since January 1, 1918 with the following results: The number of deaths during 1918 from these causes were 11. In 1919 the number was eight. In 1920 it was 7 and during the first eight months of 1921 there were six deaths which had occurred during that period and which had already been settled at the end of the period. There may have been one or two more among the unsettled cases. These deaths included such causes of death as intoxication, alcoholism, alcoholic coma, delirium tremens, dipsomania, drunkenness, ethylism and inebriety. They did not include alcoholic cirrhosis or alcoholic paralysis, nor deaths caused by a fall while in an intoxicated condition, nor deaths due to the use of wood alcohol. These latter deaths form in themselves an interesting record. During the year 1918 we had one death from wood alcohol poisoning. During the year 1919, there were 10 such deaths. During the year 1920 there were 2; and during the first eight months of 1921 there were 2.

Glycosuria:

In our experience this has caused a much higher mortality than albuminuria, and a subject with present evidence of it, or giving a past history is looked upon with grave suspicion; hence we endeavor to use every possible precaution to determine the authenticity of the specimen and to obtain it at the high tide of digestion, following an appropriate meal of at least an average amount of carbohydrates. With a history or finding of sugar, we always test for acetone and diacetic acid, and when either is found we prefer to defer the risk or decline it altogether.

Past History of Tuberculosis:

The authors have listed a history of lung trouble as coming next in the order of frequency of glycosuria. It, therefore, occurred to us that it might be of value if we would state the

ratings used by our Company in cases showing a history of tuberculosis of the lungs. It is to be remembered that these ratings, in so far as they vary according to the build of the applicant, are based on the Equitable's Build Table already referred to. The following schedule shows the ratings and the definition of the cases to which they are applied.

History of Tuberculosis of Lungs:

Phthisis, hæmoptysis (not traumatic); pneumonia—few bacilli found and went away for convalescence; lungs affected; dry pleurisy and went away for convalescence. Chronic cough (no bacilli) and sanitarium for precaution, suspicious apices, prolonged expiration, etc.

A. Without any physical signs.

	−20%	−10%	0	+10%	+20% and Over
Within 2 years. . all ages	R. N. A.	R. N. A.	R. N. A.	R. N. A.	R. N. A.
2 to 5 years. . . . age 25	+R. N. A.	+175	+125	+85	+75
35	+180	+155	+110	+75	+55
45	+125	+ 90	+ 65	+45	+30
55	+ 70	+ 50	+ 35	+25	+15
5 to 10 years. . . . age 25	+170	+120	+ 85	+55	+50
35	+145	+100	+ 75	+50	+40
45	+ 85	+ 60	+ 45	+30	+20
55	+ 45	+ 35	+ 25	+15	+10
After 10 years. . .age 25	+ 85	+ 60	+ 40	+30	+25
35	+ 70	+ 50	+ 35	+25	+20
45	+ 40	+ 30	+ 20	+15	+10
55	+ 20	+ 15	+ 10	+10	+ 5

B. Physical signs such as small dry crepitant rales, old pleuritic friction sounds, area of diminished breathing, or area of lost vesicular breathing, area of bronchial breathing, or signs of old arrested focus, moderate emphysema, if clearly pertaining to an arrested process.

	−20%	−10%	0	+10%	+20% and Over
Within 2 years. ..all ages	R. N. A.	R. N. A.	R. N. A.	R. N. A.	R. N. A.
2 to 5 years. . . .age 25	+R. N. A.	+R. N. A.	+175	+120	+105
35	+R. N. A.	+R. N. A.	+155	+110	+ 85
45	+175	+125	+ 90	+ 60	+ 40
55	+ 95	+ 70	+ 50	+ 35	+ 20
5 to 10 years. . . .age 25	+R. N. A.	+170	+120	+ 80	+ 70
35	+195	+145	+100	+ 75	+ 55
45	+120	+ 85	+ 60	+ 45	+ 30
55	+ 65	+ 45	+ 35	+ 25	+ 15
Atter 10 years. . .age 25	+135	+ 95	+ 65	+ 45	+ 40
35	+120	+ 85	+ 60	+ 40	+ 30
45	+ 70	+ 50	+ 35	+ 25	+ 15
55	+ 40	+ 30	+ 20	+ 15	+ 10

History of Pleurisy:

The history of pleurisy is so closely allied to the history of lung trouble that we also incorporated our ratings for the cases showing this history. It must be admitted that these ratings are to a certain extent based on individual judgment supplementing experience, but they are presented for whatever they are worth.

History of pleurisy. Dry or with effusion.

	−30%	+20%	+10%	0	+10%
Within 2 years. . .age 25	+90	+70	+45	+25	+15
35	+70	+50	+30	+20	+ 5
45	+40	+25	+20	+ 5	0
2 to 5 years. . . . . .age 25	+70	+55	+35	+20	+10
35	+55	+40	+25	+15	+ 5
45	+30	+20	+15	+ 5	0
5 to 10 years. . . . .age 25	+35	+30	+20	+10	+ 5
35	+30	+20	+15	+10	+ 5
45	+15	+10	+10	+ 5	0

Empyæma. About two-thirds rating for pleurisy.

Old adhesions without history of pleurisy . . . +15 to +200.

BUILD TABLE

HEIGHT			UNDER WEIGHT					STANDARD										OVER WEIGHT							HEIGHT
5 ft. 0	77	84	90	97	103	110	116	123	129	135	142	148	155	161	168	174	181	187	194	200	206	213	219	226	5 ft. 0
1	79	85	92	98	105	111	118	124	131	138	144	151	157	164	170	177	183	190	197	203	210	216	223	229	1
2	80	86	93	100	106	113	120	126	133	140	146	153	160	166	173	180	186	193	200	206	215	219	226	233	2
3	82	88	95	102	109	116	122	129	136	143	150	156	163	170	177	184	190	197	204	211	218	224	231	238	3
4	84	91	98	105	112	119	126	133	140	147	154	161	168	175	182	189	196	203	210	217	224	231	238	245	4
5	86	94	101	108	115	122	130	137	144	151	158	166	173	180	187	194	202	209	216	223	230	238	245	252	5
6	89	96	104	111	118	126	133	141	148	155	163	170	178	185	192	200	207	215	222	229	237	244	252	259	6
7	91	99	106	114	122	129	137	144	152	160	167	175	182	190	198	205	213	220	228	236	243	251	258	266	7
8	94	102	110	118	126	133	141	149	157	165	173	181	188	196	204	212	220	228	236	243	251	259	267	275	8
9	97	105	113	122	130	138	146	154	162	170	178	186	194	203	211	219	227	235	243	251	259	267	275	284	9
10	100	109	117	125	134	142	150	159	167	175	184	192	200	209	217	225	234	242	251	259	267	276	284	292	10
11	103	112	120	129	138	146	155	163	172	181	189	198	206	215	224	232	241	249	258	267	275	284	292	301	11
6 ft. 0	107	116	125	134	142	151	160	169	178	187	196	205	214	223	231	240	249	258	267	276	285	294	303	312	6 ft. 0
1	110	120	129	138	147	156	166	175	184	193	202	212	221	230	239	248	258	267	276	285	294	304	313	322	1
2	115	124	134	143	153	162	172	181	191	201	210	220	229	239	248	258	267	277	287	296	306	315	325	334	2
3	118	128	138	148	158	167	177	187	197	207	217	227	236	246	256	266	276	286	296	305	315	325	335	345	3
4	122	132	142	152	162	173	183	193	203	213	223	233	244	254	264	274	284	294	305	315	325	335	345	355	4
PCTG.	−40	−35	−30	−25	−20	−15	−10	−5	STD	+5	+10	+15	+20	+25	+30	+35	+40	+45	+50	+55	+60	+65	+70	+75	PCTG.

MORTALITY RATINGS

AGE																									HEIGHT
15	145	135	125	115	110	105	100	95	95	95	100	105	110	120	130	140	150	160	170	180	190	205	215	230	5 ft. 0
20	145	135	125	115	110	105	100	95	95	95	100	105	110	120	130	140	150	160	170	180	190	205	215	230	1
25	140	130	120	115	110	105	100	95	95	95	100	105	110	120	130	140	150	160	175	185	195	210	220	235	2
30	130	125	120	115	110	105	100	95	95	95	100	105	110	120	130	140	150	165	175	185	195	210	220	235	3
35	125	120	115	110	105	100	100	95	95	95	100	105	115	125	135	145	155	170	180	190	200	215	225	240	4
40	120	115	110	105	100	100	100	95	95	95	100	105	115	125	135	145	155	170	180	190	200	215	225	240	5
45	115	110	105	100	100	100	100	95	95	95	100	105	115	125	135	145	155	170	180	195	205	220	230	245	6
50	115	110	105	100	100	100	100	95	95	95	100	105	115	125	135	145	155	170	180	195	205	220	230	245	7
55	110	105	100	100	100	100	100	95	95	95	100	105	115	125	135	145	155	170	180	195	205	220	230	245	8
60	110	105	100	100	100	100	100	95	95	95	100	105	115	125	135	145	155	175	185	200	210	225	235	250	9
												110	120	130	140	150	160	180	190	205	215	230	240	255	10
												110	120	130	140	150	160	180	190	205	215	235	245	260	11
												110	120	135	145	155	165	185	195	210	220	240	250	265	6 ft. 0
												110	120	135	145	155	165	185	195	215	225	245	255	270	1
												110	125	140	150	160	170	190	200	220	230	250	260	275	2
												110	125	140	150	160	170	190	200	220	230	250	260	275	3
												110	125	140	150	165	175	195	205	225	235	255	265	280	4

ADDITIONAL RATING FOR ONE CONSUMPTIVE IN FAMILY HISTORY

Age										
15	60	50	40	35	30	25	20	10	10	5
20	55	50	40	30	25	20	15	10	10	5
25	45	40	35	25	20	15	15	10	5	5
30	40	35	30	25	15	10	10	5	5	0
35	30	25	25	20	15	10	5	5	5	0
40	25	20	20	15	10	5	5	0	0	
45	20	15	10	10	10	5	0			
50	15	10	5	5	5	0				
55	10	5	5	0	0					
60	10	5	0							

ADDITIONAL RATING FOR TWO CONSUMPTIVES IN FAMILY HISTORY

Age											
15	80	70	65	55	40	30	25	15	10	5	0
20	80	70	60	50	40	30	20	15	10	5	0
25	65	60	50	45	35	25	20	15	10	5	0
30	60	55	45	40	30	20	15	10	5	0	
35	50	45	40	35	25	20	15	10	5	0	
40	40	35	30	25	15	15	10	5	0		
45	30	25	20	15	10	10	10	5	0		
50	25	20	15	10	5	5	5	0			
55	20	15	10	5	5	5	0				
60	15	10	5	0	0	0					

ADDITIONAL RATINGS FOR THREE CONSUMPTIVES IN FAMILY HISTORY

Age / Pctg.	−35	−30	−25	−20	−15	−10	−5	Std	+5	+10	+15
15	90	80	75	60	50	35	25	20	15	10	5
20	90	80	70	60	50	35	25	20	15	10	5
25	75	70	60	55	45	35	25	20	15	10	5
30	70	65	55	45	35	25	20	15	10	5	0
35	60	55	45	40	35	25	20	15	10	5	0
40	45	40	35	30	25	20	15	10	5	0	
45	35	30	25	20	20	15	10	5	5	0	
50	25	20	15	10	10	5	5	5	0		
55	20	15	10	5	5	5	0	0			
60	15	10	5	0	0	0					

RATINGS FOR FAMILY HISTORY TO BE USED WITH BUILD TABLE

1. Great longevity in family .... −10 to −15
2. Modern longevity in family .... − 5 to −10
3. Average longevity in family 0
4. Great or modern longevity except one death from causes mentioned below under age 60 0
5. Absence of longevity (no deaths from causes mentioned below under age 60) .... + 5 to +15
6. One death under age 60 from causes below 0
7. Two or more deaths under age 60 from causes below or any combination of them .... + 5 to +15

Apoplexy or Paralysis, Insanity, Diabetes, Suicide, Cancer

CORRECTION TO BUILD RATINGS

Endowments Maturing Under Age 55

Per Cent Overweight

Nest. Age	15	20	25	30	35	40	45	50	55	60	65	70	75
15	0	−5	− 5	−10	−10	−10	−15	−15	−15	−20	−20	−20	−20
20	0	−5	−10	−10	−15	−15	−15	−20	−20	−20	−25	−25	−25
25	−5	−5	−10	−10	−15	−15	−15	−20	−20	−25	−25	−25	−25
30	−5	−5	− 5	−10	−10	−10	−10	−15	−15	−15	−15	−15	−15
35	0	−5	− 5	− 5	− 5	−10	−10	−10	−10	−10	−10	−10	−10
40	0	0	0	0	− 5	− 5	− 5	− 5	− 5	− 5	− 5	− 5	− 5

For Endowments Maturing at 55 to 59 inclusive, allow 5 points LESS.
For Endowments Maturing at 60 or over, allow 10 points LESS.

CORRECTION FOR AGES 30 AND UNDER

20 Per Cent or More Underweight

5 ft. 0 in. −15	5 ft. 2 in. −10	5 ft. 4 in. −5
6 ft. 0 in. + 5	6 ft. 2 in. +10	6 ft. 4 in. +15

ALLOWANCE FOR DISTRIBUTION OF BUILD

ORDINARILY NO CREDITS FOR WOMEN

Excessive Abdomen	20 to 30 Per Cent Overweight			35 to 40 Per Cent Overweight			Over 40 Per Cent Overweight		
	Age 40	Ages 40–50	Ages 50+	Age 40	Ages 40–50	Ages 50+	Age 40	Ages 40–50	Ages 50+
+4 in.	+10	+15	+25	+20	+30	+40	+25	+50	+75
+3 in.	+ 5	+10	+15	+10	+20	+25	+15	+35	+55
+2 in.	0	+ 5	+10	+ 5	+10	+15	+10	+20	+35
+1 in.	0	0	+ 5	0	+ 5	+10	+ 5	+10	+20
0	0	0	0	0	0	+ 5	0	+ 5	+10
−1 in.	0	0	0	0	0	0	0	0	0
−2 in.	0	0	0	0	0	− 5	0	− 5	−10
−3 in.	0	0	− 5	0	− 5	−10	− 5	−10	−15
−4 in.	0	− 5	−10	− 5	−10	−15	−10	−15	−20

RATINGS FOR PLAN OF INSURANCE

Endowments 20 years or less, maturing under age 65	−10
Endowments, not included above, maturing under age 70	− 5
Limited Payment Life, 20 years or less, paid up before age 65	− 5
Ordinary Life, or Limited Payment Life, or Endowment, not included above	0
Term	+15

WEIGHT CORRECTION—WOMEN

Where a woman applicant is above the STANDARD WEIGHT, add the following correction to the actual weight before entering the BUILD TABLE.

Age	Add
20	5 lbs.
25	8 lbs.
30	8 lbs.
35	6 lbs.
40	4 lbs.
45	2 lbs.

DEAR DOCTOR

Referring to your examination of the above named applicant will you kindly re-examine the Heart, fill out the blank and

complete the diagram on the reverse side of this letter, and return the same to the Society in the enclosed envelope.

Thanking you in advance for your prompt attention to the matter we are,

Very truly yours,

T. H. Rockwell, M. D.,

Medical Director.

It is Very Important from our standpoint to differentiate between *Organic* and *Non-Organic* Murmurs. It is therefore well, before reporting, to bear in mind the following general characteristics of the common murmurs.

The murmurs described below are arranged in the order of the frequency of their occurrence.

ORGANIC

Mitral Regurgitation. Systolic. Maximum intensity at apex, transmitted to axilla, heard behind at angle of scapula.

Mitral Obstruction. Presystolic, running into first sound. Heard in mitral area. Not transmitted. Usually accompanied by a thrill along left margin of heart area.

Aortic Regurgitation. Diastolic. Replaces or follows the second sound. Maximum intensity at second right interspace to third left and downwards to ensiform cartilage.

Aortic Obstruction. Systolic. Maximum intensity at right second interspace close to sternum. Transmitted upwards into great vessels of the neck.

NON-ORGANIC

These murmurs may be divided into

Functional of Several Origins—Usually heard in second left interspace region and systolic in time.

Hæmic—Due to anæmia or changed viscosity of blood. Usu-

ally heard over precordia and great vessels. Systolic in time. General condition of applicant important.

RESPIRATORY—Due to variation of intrathoracic pressure due to respiration. Usually heard at apex and disappears when breathing is arrested.

Please indicate on the diagram

1. Position of the apex impulse by (circle)
2. Area over which the murmur is heard by (dotted line)
3. Point where murmur is heard loudest by (cross)
4. If murmur is clearly transmitted from the point of maximum intensity indicate in what direction transmission is noticeable by (arrow)

Please answer the following questions as fully as possible

5. In point of time, is the murmur systolic, presystolic, or diastolic?	
6. What is your diagnosis of the Lesion?	
7. Are there any changes in the *character* of the pulse or heart sounds after exercise?	
8. Is there evidence of full compensation and if not what are the signs of incomplete or failing compensation, if any? (such as dyspnœa, œdema, or cyanosis, dilatation, etc.)	
9. Is there *true* cardiac hypertrophy, and if so, to what extent? (Be careful to locate the apex impulse, as above described, accurately.)	

10.	Aside from the cardiac condition, do you find the applicant in first class physical condition?	
11.	What is the Blood Pressure?	Systolic .... Diastolic ....

....
Medical Examiner.

....
Name of Applicant

Date........................

Dear Doctor

Subject

Referring to your examination of the above-named applicant we note your findings as to his blood pressure were as follows
Systolic............Diastolic............
Will you kindly comply with request

Request "A" Please take more blood pressure observation after a careful study of the instructions contained in this letter and forward the results to us as promptly as possible. Please use the reverse side of this sheet for reply.

Request "B" Please study carefully the instructions contained in this letter as to the method of making blood pressure observations, especially the diastolic and follow them as closely as possible in the future.

Experience has shown us that there are a number of factors, such as excitement, apprehension, fatigue, beginning of digestion, etc., which affect a blood pressure reading. If there is much deviation from the normal, we suggest, in order to eliminate nervousness, that you take a second observation

after a period of rest. If the abnormal pressure is not due to excitement, another reading should be taken on another day.

Yours very truly,
T. H. Rockwell, M.D.,
Medical Director.

When examining an applicant for the first time, should you find the Systolic pressure to be more than 10 points higher than that given in the column marked "High Systolic," please take a later observation and report all findings.

SYSTOLIC

Technique. All observations should be made by the Auscultatory Method. This is done by listening with the stethoscope placed at the bend of the elbow over the brachial artery. Air is then pumped into the cuff until the pressure stops the pulse and no sound is heard with the stethoscope. Allow the air to escape slowly. The instant the sound reappears on deflating the cuff is that at which the dial should be read as marking the Systolic pressure.

Age	High Systolic	Average Systolic	Average Diastolic	Average Pulse Pressure
20	137	120	80	40
30	140	123	82	41
40	144	126	84	42
50	148	130	86	44
60	154	135	90	45

DIASTOLIC

By continuing to deflate the cuff, a series of murmurs and tapping sounds are heard, gradually becoming less distinct, finally disappearing altogether. We wish to have the diastolic

pressure recorded just at the point at which all sounds disappear.

Our experience shows greater accuracy in taking the systolic than the diastolic pressure. We suggest, therefore, that our Examiners thoroughly acquaint themselves with the above simple method.

The diastolic pressure cannot be accurately obtained without the use of the stethoscope.

Our belief is that a normal DIASTOLIC pressure rarely if ever exceeds 100 mm.

Your instrument should be tested from time to time for accuracy.

Name of Applicant..........

1. Date........192....... o'clock A.M. P.M. — Systolic (maximum) pressure .... Diastolic (minimum) pressure ....

2. Date........192....... o'clock A.M. P.M. — Systolic (maximum) pressure .... Diastolic (minimum) pressure ....

3. Aside from the blood pressure, do you find the applicant in good health?

....
MEDICAL EXAMINER.

Dr. Toulman—Again we are greatly indebted to Mr. Hunter and Dr. Rogers for a contribution of the greatest importance to the literature of Life Insurance selection. Based on their unusually wide experience, tempered with excellent judgment, this paper and others similar to it which they have published in quite recent years, must be given first honors. Nor must we lose sight of the generosity of the company they represent in permitting them to publish results which must represent such a large outlay of both time and money. I feel peculiarly indebted to them, because the Penn Mutual has concluded to write underaverage business. The ratings which the authors

have published will naturally be of the greatest help to us. Indeed, were it not for their contributions, I believe we would not feel in a position to safely undertake this form of underwriting.

As I recently had occasion to summarize by cause the number of cases declined and postponed by our Company the first three months of the year, I present the following table. It does not include cases in which more than one impairment was found, nor those found only once. They are arranged in the order of their frequency.

Albumin in the Urine
High Blood Pressure
Heart Murmur
Overweight
Unsatisfactory habits as to alcohol, past or present
Tuberculosis in Family History,—usually young, light-weight, applicants
Sugar in the Urine
Casts in the Urine
Tuberculosis, Personal History of
Light Weight
Asthma, History of
Renal Colic, History of
Syphilis, History of
Pulse, Rapid or Irregular
Goiter
Gastric or Duodenal Ulcer, History of
Pleurisy, History of
Hepatic Colic, History of

Seventy-five per cent. of the entire number were included in the first six, and, it will be noted, five of these are found in the first six of the authors' list. The comparison of the two lists is interesting even if not of particular value.

In expressing the rating by the excess mortality above the normal that must be expected, the authors are to be commended. It is stated that changes have been made in some of the ratings, as would be expected. It would be interesting to know in which classes it was found necessary to increase, and in which to lessen, the ratings.

Heart Murmur. The necessity for individualizing the ratings is well exemplified in the remarks on heart murmurs. I have always thought that it was this feature of substandard work which must hold the greatest interest and fascination to the Medical Board, and which calls for the widest experience and maturest judgment possible.

The important part that the degree of hypertrophy seems to play in the New York Life ratings is of especial interest. Hypertrophy so frequently—I almost said constantly—accompanies valvular lesions[1] that I believe it is reasonable to assume the very favorable mortality in the class "Very Best, No Hypertrophy," is in part due to faulty diagnosis,—a murmur being responsible for a diagnosis of valvular defect which may not exist. Osler's statement in connection with his writing on the subject of aortic obstruction bears out this idea.

I should like to ask how frequently Dr. Rogers calls for the opinion of an expert in heart cases, and how helpful he finds the diagram of the chest, used in these cases.

Overweight. Notwithstanding the fact that all the companies have access to the tables published by companies, individually and collectively, showing the influence of build on mortality, there continues to be a very wide divergence of action, with a marked tendency, as I see it, to optimism.

If fifty companies carefully studied their own individual experience I wonder if the findings would not make a deeper impression.

Albuminuria. I have often spoken of the importance of each company knowing its own experience, and am glad to see this idea mentioned by the authors in connection with their remarks under Albuminuria.

[1] Babcock. "If the heart did not respond to the demands for extra effort by the development of hypertrophy, its accommodative power to diseased conditions would soon reach its limit. Consequently cardiac hypertrophy may be regarded as a conservative process."

Aortic regurgitation.
One of tne first signs is hypertrophy.

Aortic stenosis.
Hypertrophy is the first result.

Rapid Pulse. As Osler points out, rapid heart action may be perfectly normal in certain individuals. In nervous individuals the mere thought of an examination accelerates the pulse. These facts make more striking the necessity for the high rating called for. In connection with the significance of rapid pulse I would add goiter. In so many instances it is the rapidity of the heart which first calls the clinician's attention to the thyroid.

Slow Pulse. While I am inclined to believe that a moderately slow pulse is not a disadvantage, I think we should have much more data upon which to base our conclusions before deciding that a pulse below fifty or fifty-five is not accompanied by high mortality.

Blood Pressure. We all share with the authors the belief that we cannot, as yet, draw positive and final conclusions as to the limits, both high and low, between which both systolic and diastolic readings may be considered normal. They have given us ratings, subject to change, and that is a beginning. It is my belief, based on meager statistics, observations and inquiry, that the maximum systolic should not be above 150 mm. even at the older ages, if we would have a favorable mortality, and on the other hand, readings somewhat below the minimum may, as a rule, be safely accepted.

Syphilis. I heartily concur with the ratings necessary to meet this impairment, and the necessity of considering as uninsurable those who have had tertiary symptoms. I presume that those who have had the initial lesion only require a comparatively small rating because among them are a certain number of cases in which a wrong diagnosis was made.

Gastric and Duodenal Ulcer. With so little data on this subject upon which to base conclusions, the authors wisely advise us to be conservative. I thoroughly agree with their warning as to the outcome of cases operated upon by men less efficient, thorough, and experienced than the surgeons of the Mayo Clinic. We certainly should have an experience covering a much longer period,—an exposure of not less than twenty years, I believe,—before feeling satisfied. I cannot agree with

the belief that the extra hazard may be assumed to be a temporary one. Not only have we the danger, in later life, of cancer, but we have an artificial canal. As one of my associates puts it,—"You can't improve on God Almighty's plumbing." Not only should stress be laid on the ability of the operating surgeon, but great importance should, we think, be placed on the history of the presence or absence of indigestion, subsequent to the operation. We do not agree with the assumption that a case of gastric ulcer, not operated upon, is as favorable as that which has gone to successful operation by a first-class man, but do agree with the idea as to the higher mortality to be expected when duodenal ulcers have not gone to operation.

Gall Stones. We are in agreement with the writers as to the decreasing hazard which results from the increasing elapsed time, but believe that ages under thirty-five may be safely taken at a rating perhaps one-half that of the older ages. We have some doubt as to the favorable attitude towards cases in which the gall bladder has been removed, unless the applicant is likely to have the very best surgical attention if a subsequent operation becomes necessary. As with the ulcer cases, so in this class we think the subsequent digestive history to be of great importance.

Renal Colic. Will Dr. Rogers kindly tell us if he has ever made a study of cases giving a history of having passed a stone, with subsequent negative X-ray findings? The data are, I fear, too meagre for separate study. Is the treatment of such a case that of any other applicant giving the usual history of renal colic?

Sugar. It may be of interest to know that we have made use of the test meal, as described in the text, for many years, with very satisfactory results. In many instances we duplicate the meal which preceded the finding of sugar. From the experience of the New England as reported by Dr. Dwight, the most careful selection should be practiced to warrant any other than a high rating.

While the tables given under the various headings constitute,

of course, the most important part of the paper, I consider the warning as to their proper interpretation and their application to be of almost equal importance. Let us not forget that "these ratings should not be looked upon as final but only as expressing the best information now available on the subjects to which they apply"; and that "they should not be looked upon as absolute ratios of extra mortality, but, rather, as reasonable guides in the selection of risks for insurance."

Dr. Weisse—I have read with a great deal of interest "The Ratings for the Principal Impairments" presented by Dr. Rogers and Mr. Hunter, and wish to express my hearty appreciation of, and admiration for the work that they have done along this line and of their generosity in publishing their results for the use of other companies.

The Mutual Life has comparatively few records for comparison as we do not do business at other than standard ratings. Through the courtesy of Doctor Symonds, I am able to present some statistics of our own business which may, however, be of interest.

All ratios are obtained in the same manner as in the Medico-Actuarial Mortality Investigation. I also give the actual excess mortality on our general experience of 75%, and the same excess on the 100% base for purposes of comparison.

BLOOD PRESSURE

Since 1907 we have kept a careful record of systolic, and since 1917, of both systolic and diastolic blood pressures—taken by Medical Directors and Home Office Examiners, with the hope that, after a longer period of exposure, we may obtain some valuable information. These readings were all taken under identical circumstances at the Home Office, by men carefully trained to make their readings at the same point. The limits for the systolic were 100 to 150, under age 45, and 100 to 160, over age 45; for the diastolic, 105; for pulse pressure, 55.

Diastolic reading taken at end of fourth phase.

The following table shows the results of these records—all taken on men recommended for insurance by the Medical Examiner:

Ages	Systolic Blood Pressure		Diastolic Blood Pressure		Pulse Pressure
	No. of Cases	Averages	No. of Cases	Averages	Average
15–19	433	121.1 mm.	215	78.2 mm	42.2 mm.
20–24	1,658	124.1 "	603	79.6 "	42.4 "
25–29	2,332	124.7 "	901	80.9 "	42.5 "
30–34	2,192	124.5 "	1005	82.0 "	41.3 "
35–39	1,889	125.7 "	876	83.0 "	41.8 "
40–44	1,466	127.5 "	642	84.0 "	42.3 "
45–49	961	129.1 "	424	84.8 "	43.1 "
50–54	567	131.5 "	250	85.8 "	45.4 "
55–59	262	134.5 "	103	86.1 "	47.8 "
60 & Ov.	168	134.8 "	71	84.9 "	50.2 "
Total	11,928	126.0 "	5090	82.3 "	42.5 "

It appears that, while not knowing on how many actual blood pressure readings Doctor Rogers has worked out his averages, or under how similar circumstances these were taken, the coincidence of the averages obtained from the two series of readings makes it fairly certain that they represent the average blood pressures to be expected in healthy males at the given ages.

HABITS IN THE USE OF ALCOHOL

We have tabulated our experience on "daily drinkers—but not to excess"—(issues of 1907–1917—exposed to 1918) with considerable detail. In this class we have men who admit that they drink daily but not to excess, and have checked their statements by the report obtained when the policy was issued,

and by our inspector's report on each case, made within two years of issue—throwing out all cases who are not daily drinkers or who give any history of ever drinking to excess. I think this group is about the same as Dr. Rogers's—"Steady Free Users (a)."

For purposes of comparison, we have divided the domestic field into five areas:

1. Northern tier of the Southern States, comprising Virginia, West Virginia, Kentucky, Tennessee, No. Carolina.
2. Southern tier of the Southern States, comprising South Carolina, Georgia, Florida, Alabama, Mississippi, Louisiana, Arkansas, and Oklahoma.
3. Other States east of the Mississippi.
4. Other States west of the Mississippi.
5. Canada.

The mortality of this group was higher than the average in all parts of the country.

Area	Entrants	Deaths	Ratio	General Experience	Actual Excess	Excess in Terms of 100%
1	2,194	81	99%	76%	(23)	+32
2	1,483	60	118%	91%	(27)	+30
3	19,873	515	82%	70%	(12)	+17
4	4,003	129	108%	78%	(30)	+38
5	527	22	109%	92%	(17)	+18
Total	28,080	807	89%	75%	(14)	+19

The light weights (Group 7–8) showed a mortality of 144%, the average weights (Group 6–0–1) a mortality of 86%, and the heavy weights (Group 2–3–4–5) a mortality of 89%. The mortality of the daily drinkers at the different ages showed as follows:

Ages	Entrants	Deaths	Ratio	General Experience	Actual Excess	Excess in Terms of 100%
15–19	293	6	114%	84%	(30)	+36
20–29	7,908	125	84%	73%	(11)	+15
30–39	10,574	212	88%	73%	(15)	+20
40–49	6,718	246	94%	77%	(17)	+22
50–59	2,261	161	87%	74%	(13)	+18
60 & Ov.	326	57	91%	77%	(14)	+18
Total	28,080	807	89%	75%	(14)	+19

There were 93 deaths from tuberculosis in this group; 17 among the light weights, 76 among the middle weights, and none among the heavy weights. Cirrhosis of the liver caused 12 deaths; 7 among the average weights (.7 per 10,000 exposed) and 5 among the heavy weights (2.5 per 10,000 exposed).

GALLSTONES, BILIARY OR HEPATIC COLIC

These were applicants giving a history of one or more attacks of gallstones, biliary or hepatic colic: all treated medically—none accepted until at least five years had elapsed since the last attack:

MEDICAL IMPAIRMENT—CLASS 12. (ISSUES OF 1907–1917—EXPOSED TO 1918)

Entrants	Actual Deaths	Expected Deaths	Ratio	General Experience	Actual Excess	Excess in Terms of 100%
415	16	14.8	107.7	75%	+33	+44

RENAL COLIC, GRAVEL OR CALCULUS

Dr. Rogers gives the Mutual Life experience, 1885–1906, for renal colic, gravel or calculus as—approximately—110% M. A. selected. In 1908 we began to accept cases with a his-

tory of one or more attacks of renal colic, gravel or calculus, six months after the last attack under age 40, and one year after the last attack over age 40, provided that applicant was under 20% overweight, had a satisfactory blood pressure, a satisfactory statement from the physician who took care of him during his last attack, a negative microscopical examination of urine of good specific gravity, made either at the Home Office or by the Medical Referee, and no other *important* impairment.

MEDICAL IMPAIRMENT—CLASS 24. (ISSUES OF 1907–1917—EXPOSED TO 1918)

Entrants	Actual Deaths	Expected Deaths	Ratio	General Experience	Actual Excess	Excess in Terms of 100%
1093	36	41.6	86.5	75%	+11.5	+15

This experience gives, so far, more satisfactory results than our previous investigation, but it remains to be seen what the next ten years will bring forth.

SYPHILIS

At the twenty-ninth annual meeting at Newark in January, 1919, I quoted our experience of syphilis based on investigation of issues of 1885 to 1906, exposed to 1915, and I will repeat our experience briefly.

Medical Impairment	Entrants	Exposures	Deaths	Ratio	Excess
Class 1	176	1631	18	135.5	+35.5
" 2 & 3	134	1261	18	137.5	+37.5
" 4	122	1121	17	161.	+61.
" 1, 2, 3, 4 (combined)	432	4013	53	143.6	+43.6

The excess in this Table is based on the 100% general experience.

I am glad to say that our experience, unsatisfactory as it was, was apparently better than the average of those mortalities quoted by Doctor Rogers.

Our experience with reference to albuminuria will be considered by Doctor Russell in his paper, so I will not refer to that here.

I am sorry that we have no actual data on impaired risks to present for comparison but I hope that the few facts that I have obtained from our records may be of interest.

I am glad to have this opportunity to thank Dr. Rogers and Mr. Hunter for their distinctly valuable contribution to the science of sub-standard medical selection.

Dr. Eakins—To discuss a Rogers-Hunter paper presupposes a temerity which is not inherently mine. A sufficiency of complimentary phraseology, all of it well-deserved to be sure, has been directed toward them the past few years. Unless I indulge in oft repeated expressions that avenue of discussion is closed. The path of criticism remains open. Had I the audacity to enter there, I find it barricaded with an objection which keeps me out: I have no business to transact up that way at all, and I cannot see that a journey in that direction would be any joy ride. It's a bumpy road! Anyone who has traveled it, knows it is!

If we are keenly ambitious we do not confine ourselves to praise of the man whom we admire. We study, and try to emulate him. It depends upon ourselves whether we accomplish merely an imitation, or otherwise. We are bound to admire one who creates, if we realize how difficult an art creating is. Like other criticism that of his creation is so easy as to be a pastime, sometimes thoughtless and puerile. If we have any worthwhile criticism it is bound to be constructive. In its absence, silence is the noblest method of expression. I am timorously willing to violate the silence, at the risk of being ignoble. How much credence can be placed in an individual who denies a medical history, previously admitted? Our inter-

pretive position would be strengthened if the progenitors of the Numerical Rating System would give us a percentage basis, from which to work, to cover liars. Sad as it may be, there are such, more than enough to warrant the need for a basic rating. If the truth were known perhaps there are more liars than heart murmurs. Both are hazards, and, although we have a rating for only one, we have symbols in the M. I. B. for both. That, whimsically if you will, constitutes constructive criticism, consisting of an appeal to Rogers and Hunter to extend the scope of their fundamentals to cover features beyond Occupational and Medical ratings.

If such a silence breaking request can be considered sophistical or ignoble, what would the criticism of Numerical Ratings which is heard most frequently be called? It seems the oftenest sung refrain of aspersion is, "The Numerical Method is not scientific." Whether it is or not, it may be pertinent to ask those who thumb on that one string to explain what they mean by "scientific." To be scientific do they require something which shall be accurately exact, or exactly accurate? Is the art of medicine scientific? Is the actuarial art scientific? According to the definition in the book, almost anything may be, or nothing is, scientific. There are few subjects concerning numerous aspects of which there are not differences of opinion. Wherever more than one interpretation is possible the essential fundamentals cannot be either accurately exact, nor exactly accurate. If we are to be so meticulous in our definitions, then I agree that the Numerical Method is unscientific. I affirm further that it is, as such, in good company: with, for example, the Laws of Newton which were highly popular in scientific circles and served their purpose moderately well for three hundred years. They do not yet seem to be labeled as "unscientific," even by Einstein himself. I hold no brief for the Rogers-Hunter combination, nor the Dictionary either for that matter, but I do feel constrained to defend the Numerical Method against a misuse of terms. As generally understood, when a thing is called unscientific, that thing is airily waived beyond the pale of recognition, casually relegated to that

limbo of junk where rusty oblivion reigns. How absurd it is to consider anything to be theoretically possible, but not practicably so. If the theory is sound, obviously the practice must be feasible. If "it," whatever it may be, is a failure practically, then the theory has flaws. If that premise is true, the reverse obtains. The Numerical Method has proven its practicability. Being practically sound, its theory, of necessity, is healthy. If the problem, both in theory and practice, works out the answer is in no need of a physician's attention; no call for an ambulance is indicated; it is not a hospital case. Going strong and being sturdily robust possibly it can stand being called names, even unscientific.

I sincerely trust I have not inadvertently touched upon a point which Rogers and Hunter may have wished to handle themselves, or ignore. The opportunity afforded me by this discussion was too good a one to miss for the sake of my peace of mind. It does not profit a man to be in constant wrangle, nor agreement, with his fellows. In the former, the price is prohibitive; in the latter, life has not its proper spice: and, too, both states are monotonous, which is disturbing to mental serenity. Mental serenity itself is anathema to some people. However, it is found in greater proportion out of, than in, psychopathic subjects.

Whether or not Rogers and Hunter are barking, unscientifically, up a tree or are baying, unscientifically, at the moon, if they keep it up long enough, everybody will know everything after a while. We all hope they will keep it up; it will be eminently fortunate for the rest of us if they do.

Dr. Rogers—Both Mr. Hunter and I feel deeply appreciative of the generous treatment that has been accorded to us and, on his behalf as well as for myself, I wish to express my thanks. Even those who disagree with us have shown a degree of indulgence that we highly appreciate.

It is particularly gratifying to us to have Mr. Henderson and Dr. Rockwell contribute so valuably to the discussion of our paper, for the reason that both of these gentlemen have had an opportunity to test out in practice over a long period of

time the numerical method. I would not have you think that Mr. Hunter and I are wedded to that method. I feel sure that none of us are so bound to it that we are not quite receptive to some other if it has advantages over it.

I wish also to thank Dr. Eakins for his testimony in favor of the numerical method for he, too, has worked with it and knows its value.

I like very much Dr. Weisse's paper. His contributions are of real value. Particularly valuable is his study of the average diastolic pressures which it seems are now in a fair way to be placed side by side with the average systolic pressures. These no doubt we may use as guides to help us in our judgment of many border-line and substandard risks.

Dr. Toulmin asks if we employ experts in heart cases. We do not, excepting in so far as we happen to have experts in our service. Our reason for not employing heart experts is that our business is nation-wide. We have based our present manner of treating heart cases on the testimony of examiners throughout the country, and we believe in doing business on that testimony rather than on the testimony of experts, however scientific they may be.

These heart cases must be insured with a certain amount of caution, and the larger the risk the more cautious we must be with them. Fortunately our larger risks come from centers of population where we are able to have the benefit of examiners who have been unusually well trained in diagnosis. We do not use the X-Ray or the fluoroscope, but we frequently receive testimony of that sort and try to bring it into line with the other information we have regarding these cases.

As to habits in the use of alcohol, Dr. Weisse's contribution is a welcome addition to the very valuable testimony we already have on that subject. It supports that of Dr. Fisher, of Dr. Porter, and of Dr. Dwight, who have all made attempts to distinguish between very temperate users and moderate users of alcohol. Dr. Porter showed that if we take total abstainers as our standard of 100% mortality, very temperate, not daily users, showed a mortality of 120%, and moderate

daily users of 136%. Dr. Fisher showed total abstainers, 100%, occasional users, 119%, daily users of beer, 133%, daily users of spirits, 166%. Dr. Dwight showed total abstainers, 100%, those who rarely use, 124%, temperate users, 143%, moderate users, 213%.

These testimonies are all in line with that of the Medico-Actuarial Investigation which showed, measured by the M. A. standard, conservative daily users, 118%, liberal free users, 186%. It is unfortunate that in that investigation we made no study of total abstainers so that we cannot know what these ratios would have been, measured in terms of total abstainers accepted under the same conditions. The Medico-Actuarial Investigation showed that it probably cost the life companies at least $650,000 to insure the so-called liberal free user group in that Investigation. As to the thirty odd thousand of conservative or quite moderate users, the probable financial loss was at least $530,000.

Groups 17, 19 and 20 of the Medico-Actuarial Mortality Investigation, made up of about 13,500 lives who use alcohol to excess occasionally must have caused a loss of about $400,000; and the sixty thousand lives of persons engaged in the trade of alcoholics, a loss of two and a half million dollars.

Those who might wish to support the thesis that persons who use alcohol freely may be good risks for insurance have a great deal of adverse testimony to overcome. Indeed, I think that the evidence is conclusive that the so-called Anstie's limit of one and a half ounces, or three tablespoons of alcohol a day, is far too liberal. There appears to be no limit within which alcohol may be entirely harmless. It is as if there were a direct relation between the amount of alcohol used and the amount of damage done to the body. The evidence is strong also that the damage done persists a long time after the use of alcohol has been discontinued. Anyone who uses alcohol now or has used it in the past is a less desirable risk, all other things being equal, than a total abstainer, and his undesirability is in proportion to the freedom with which he has used the drug.

SECOND DAY

President Knight in the chair. The meeting was called to order promptly at ten o'clock A.M.

The Secretary announced that he had cast a ballot as instructed for the candidates placed in nomination on the preceding day, and the following officers and members of the Executive Council were declared elected:

PRESIDENT

DR. THOMAS F. McMAHON

FIRST VICE-PRESIDENT

DR. FRANK L. GROSVENOR

SECOND VICE-PRESIDENT

DR. WILLIAM R. WARD

SECRETARY

DR. ANGIER B. HOBBS

TREASURER

DR. CHARLES L. CHRISTIERNIN

EDITOR OF THE PROCEEDINGS

DR. ROBERT M. DALEY

MEMBERS OF EXECUTIVE COUNCIL

DR. GEORGE A. VAN WAGENEN
DR. EDWIN W. DWIGHT
DR. J. ALLEN PATTON

Dr. Knight—Dr. Dillard and Dr. Chapman of the Penn Mutual Life Insurance Company have collaborated on a paper on the subject of the standardization of medical examination blanks, and Dr. Dillard will now present this paper to the meeting.

Dr. Dillard read the following paper:

STANDARDIZATION OF MEDICAL EXAMINATION BLANKS

By Dr. H. K. Dillard and Dr. J. P. Chapman

The question of the adoption of a standard medical blank for all companies has been under consideration for some time. Papers have been presented on the subject and a great deal of informal discussion has taken place wherever the members of the Medical Directors' Association have gotten together. There can be no doubt that the consensus of opinion favors the adoption of a uniform blank, and it is therefore the object of the authors of this paper to bring the matter to a head, so that some positive action can be had rather than mere discussion.

The fact has already been accomplished by the American Life Convention and it is largely due to the action of this body that we have been stimulated to encourage similar action by our own Association.

A very excellent paper was presented to the Medical Section of the American Life Convention by Thomas W. Blackburn, their Secretary and Counsel, at their meeting in March, 1920, with the result that a Committee was appointed to take action, and the thing was accomplished within a year. Why cannot we obtain similar results?

A tabulation of the questions on the blanks of fifty-seven companies belonging to this Association shows the following extraordinary results.

The total number of questions asked is 1559, summarized as follows:

Statement to Medical Examiner.

Family History	151
Diseases of Circulatory System	19
Diseases of Digestive System	47
Deformities and Malformations	17
Drugs and Tobacco	9
General Questions	165
Diseases of Genito-Urinary System	37
Use of Liquor	27
Diseases of Respiratory System	27
Female Applicants	65

Medical Examiner's Report.

Diseases of Circulatory System	67
Deformities and Malformations	28
Drugs and Tobacco	9
General Questions	165
Diseases of Genito-Urinary System	35
Height and Weight	15
Use of Liquor	27
Diseases of Nervous System	27
Diseases of Respiratory System	27
Female Applicants	67

Report of Medical Examiner.

Diseases of Circulatory System	67
Deformities and Malformations	28
Diseases of Digestive System	41
Drugs and Tobacco	5
General Diseases and Questions	163
Diseases of Genito-Urinary System	53
Height and Weight, etc.	40
Use of Liquor	12
Diseases of Nervous System	18
Diseases of Respiratory System	54
Female Applicants	67

The questions that appear on twenty or more of the fifty-seven blanks are as follows:

Family History.

If Living	Father	Mother	Brothers	Sisters
Ages	55	55	56	56
Health	56	56	55	55
If Dead				
Ages	57	57	55	55
Cause of Death	57	57	54	54
Duration of Last Illness	52	52	53	53

If Living	Father's Father	Father's Mother	Mother's Father	Mother's Mother
Ages	29	29	29	29
Health	29	29	29	29

One Company does not ask for full family history.

Have any near relatives (grandparents, parents, brothers, sisters, uncles, aunts) had tuberculosis, heart disease, apoplexy, insanity, cancer, diabetes, hereditary disease, committed suicide? 22

Have you lived (during the past 1, 2, 3, 5 years) or are you now living in the same house or been intimately associated with a consumptive? 26

Diseases of Circulatory System.

Have you had:

Apoplexy	22
Heart, disease of	32
palpitation of	26

Diseases of Digestive System.

Have you had:

Appendicitis	35
Biliary colic	25
Diarrhœa	27
Dyspepsia	24
Fistula	37
Gallstones	22
Jaundice	22
Liver, disease of	38
Stomach, disease of	23
Have you hernia (rupture)	28
Any accident or injury	20

Use of Drugs and Tobacco.

Do you now (have you ever) used (habitually or occasionally) drugs (opium) (cocaine) (morphia) (other habit forming drugs)? 47

Have you ever taken any cure or treatment for any drug habit? 36

General Questions.

What is your full name? 26

Do you contemplate a change of residence or traveling outside of the mainland of the United States? 38

Have you ever changed climate (residence) on account of your health? 25

Have you ever been advised to change climate (or residence) on account of your health? 19

What is your date of birth?	22
Present occupation?	36
Do you (have you) contemplated any change (temporary or permanent) in your occupation (on account of your health)?	25
Are you married?	23
Are you single?	18
Are you a widow (widower)?	16
Are you now (have you ever been) insured in this Company?	46
Have you ever applied for insurance without receiving a policy as applied for (been limited) (modified)?	34
Are you (now) in good (perfect) health?	23
Have you ever applied for or received a pension, accident, or sick benefits?	20
What ailments have you had in the last 2, 5, 7, 10 yrs.?	20
Name of physician last consulted by you.	21
Have you ever undergone a surgical operation?	39

Have you ever had:

Cancer	32
Ears, discharge from	27
disease of	30
Eyes, disease of	28
Gout	49
Rheumatism	54
Skin, disease of	29
Tumor	33

Have you consulted a physician for any ailment, disease, operation, or injury not included in your above answers (in the past 7, 10 years)?	35

Diseases of Genito-Urinary System.

Have you had:

Bladder, disease of	36
Gravel	20
Kidneys, disease of	40
Syphilis	36

Have you gained or lost weight (in the last, 5, 3, 2 years) recently?	27

Use of Liquor.

Have you now, or have you had, any connection, direct, or indirect, with the manufacture or sale of wines, spirits, or malt liquors (alcoholic beverages)?	32
Do you now use wines, spirits, or malt liquors (intoxicating liquors) (to excess)?	20
What quantity do you use?	26
What is your daily consumption of liquor?	29
Do you ever use it to excess?	29
Have you ever been under treatment for the liquor habit?	45

Diseases of the Nervous System.

Have you had:

Brain, disease of	31
Convulsions	25
Dizziness	19
Epilepsy	20
Fainting	17
Fits	16
Headache	27
Insanity	19
Nervous prostration	17
Nervous system, disease of	32
Paralysis	33
Sunstroke	23
Unconsciousness	22
Vertigo	16

Diseases of the Respiratory System.

Have you had:

Asthma	37
Bronchitis	19
Cough, habitual	35
Lungs, disease of	32
Pleurisy	30
Pneumonia	20
Spitting of blood	56
Tuberculosis	19

Female Applicants.

Have you had:

Breast, disease of	19
Breast, tumor of	19
Are you pregnant?	22
Have you passed the menopause?	20

Medical Examiner's Report

Diseases of the Circulatory System.

Is there evidence of past or present disease of the heart?	38
Are (both) heart sounds clear (normal) (distinct)?	12
Is there any murmur?	13
Is there any enlargement (hypertrophy)?	19
What is the blood pressure, systolic?	55
diastolic?	42
What is the pulse rate?	56
Is the pulse intermittent?	38
Is the pulse irregular?	37
What is the quality (character) of the pulse?	26
Is there evidence of past or present disease of the blood vessels (vascular system)?	19
Is there any evidence of hardening (abnormality) of the radial or temporal arteries (arterial sclerosis) (degeneration)?	24

Deformities and Malformations.

Has applicant any:

Deformity?	30
Describe any scars or marks of identification.	25

Diseases of Digestive System.

Is there (does physical examination reveal) any evidence of past or present disease of abdomen (abdominal organs)?	21
Is there a rupture (hernia)?	37
Is a (suitable) truss (habitually) worn?	30

Use of Drugs.

Have you reason to suppose that applicant is or has been intemperate in the use of drugs?	9
Have you ever seen applicant under the influence of drugs?	6

General Diseases and Questions.

What is the applicant's apparent age?	21
How long have you known the applicant?	40
How intimately (well)?	23
Are you related to applicant or agent?	34
What is applicant's race?	45
What is color of applicant's eyes?	25
hair?	25
What is applicant's general appearance?	19
Is his general appearance healthy?	23
Have you had:	
Ear (middle), disease of	38
Eyes, disease of	25
Skin, disease (eruption)	28
What is applicant's temperature?	29
Are you and the applicant alone?	24

Surroundings.

No ten companies ask the same question in regard to the surroundings of the applicant. The nearest we come to it is the following question:

Do you know anything in connection with applicant's moral character, physical condition or past health record not already detailed which would unfavorably affect insurability?	5
Do you (unqualifiedly) recommend applicant?	22

Diseases of the Genito-Urinary System.

Analysis of urine:	
Reaction	41
Specific gravity	54
Does the urine contain albumin?	56
What test was used?	16
Does the urine contain sugar?	56

What test was used?	16
Was urine passed in your presence?	26
Was urine voided by applicant?	12

Is there evidence of:

Genito-urinary organs, disease	15
Gonorrhœa	12
Prostate, enlarged	12
Stricture	24
Syphilis	23

Height, Weight and Measurements.

What is applicant's height?	57
Did you measure applicant?	39
What is applicant's weight?	57
Did you weigh applicant?	41
Has weight recently changed?	40
What is the measurement of applicant's chest on full inspiration?	46
—— forced expiration?	46
Measurement (girth) of abdomen?	57

Use of Liquors.

Do you know or suspect (believe) applicant ever has been intemperate?	17

Diseases of the Nervous System.

Do you find evidence of past or present disease of the brain or nervous system?	33

Disease of the Respiratory System.

Are there any physical signs of (past or present) disease of the lungs?	20
Is there any evidence of past or present disease of the respiratory organs (lungs)?	17

Female Applicants.

How many children has she borne?	25
Have pregnancies and labors been normal?	20
Has she ever miscarried?	17
Has she passed the menopause?	20

The following are the only questions which appear on fifty or more of the fifty-seven blanks:

If Living	Father	Mother	Brothers	Sisters
Ages				
Health				
If Dead				
Ages				
Cause of Death				
Duration of last illness				

Have you ever had Rheumatism?
Have you ever spat blood?
What is the systolic blood pressure?
What is the specific gravity of the urine?
Does the urine contain albumin?
Does the urine contain sugar?
What is the applicant's height?
What is the applicant's weight?
What is the measurement of the abdomen?

Have you lived (during the past (1), (2), (3), (5), years (are you now) in the same house, or been intimately associated with a consumptive?

Do you (unqualifiedly) recommend the applicant or risk?

It is therefore, not so much a question as to the size of the blank or its appearance or whether or not the application is attached to the medical, as it is to see that the same questions are asked an applicant for insurance in one company as are asked when applying to another, for after all, we are trying to arrive at the same conclusion and therefore why should we not select the best method for our purpose and adopt that as uniform.

Our experience in examining in a large city, for practically all the companies, has convinced us of the necessity of making our requirements uniform. Such a change would certainly be welcomed by the applicant. The question is constantly asked, "When will Insurance Companies standardize their blanks?" We are subject to the criticism, not only of the laity, but also of the courts, whenever legal action is being brought. Anyone

who has ever had to labor through a complicated insurance examination with some of those very precise lawyers will realize what it means to have a uniform, simplified, set of questions. In their own profession they are trained to the use of legal forms that are uniform throughout and cannot understand why insurance companies do not get together on a standard method for their own work.

Standardization is the order of the day. It has come in manufacture, in business, in commerce, in education,—everywhere. The rates and forms of policies in life insurance have long ago been practically standardized. It is high time, therefore, that the medical blanks should fall in line.

Many examiners the country over, examine for a number of companies. It is most confusing to have to struggle over so many blanks with so much variation in their requirements. The more skillful the examiner the more he resents the multitude of questions and answers, and he does not hesitate to express himself freely. He will promptly damn the blank that covers two foolscap pages and unconsciously favor the Company that has the short one and he will give the latter just as reliable an opinion, if not a more reliable.

It will be observed on comparison that some companies' blanks are very long and very detailed, others quite short in comparison and much condensed, but with *no* appreciable advantage in the mortality in favor of the long blank. Why then put an examiner to the trouble of wading through an endless list of separate diseases which only annoy him and the applicant, and defeats its own end? Why ever ask questions that are not absolutely essential, and there are many such on many blanks?

It was gratifying to us to find in our investigation of the blanks of some 57 companies, members of this Association, that Medical Directors themselves unconsciously are working toward a uniform goal. We could pick out the blanks of any one of a dozen or more companies which could readily be accepted as standard. Why, therefore, do we not, instead of having this work done individually, concentrate our efforts by

adopting a set of questions as standard and then appoint a permanent medical blank committee for its future control?

The arguments in favor of a uniform blank are very clearly set forth in the report of the Committee appointed by the American Life Convention and are herewith summarized.

First.—In order to obtain rapid, complete and thorough medical reports from the examiner.

Second.—To give all companies a uniform basis of selection and uniform and comparable data from which to determine and modify their action on the risk submitted. Under our present method each company adds or subtracts questions from their blank without having the benefit of the consensus of opinion of a uniform blank which a permanent blank committee would give.

Third.—The uniform blank with its thorough and logical sequences will be a great assistance in reviewing applications at the Home Office, especially in cases of reinsurance and papers submitted or received.

Fourth.—The understanding and sympathy of the field force, as well as the examiner, with medical requirements and selection, would be greatly facilitated by the realization that the medical blank represents well considered and uniform requirements upon which the Home Office has agreed. We cannot entirely fail to understand the agent who objects to the requirements of his Company if he can point, as he takes delight in doing, to other companies of good standing which take no recognition of an item which cost him his commission. By making our requirements uniform we dignify them, establish their reasonableness and gain the whole-heartedness of our field force.

Fifth.—A permanent examination blank committee could receive suggestions in regard to new questions or other changes; could make a comprehensive investigation of the need and advisability of the change; obtain the consensus of opinion of the different members and outside sources, so that a change, if made, would prove of the greatest value.

As far as the make-up of the blank is concerned this, of

course, should be left to the individual companies. The size, for instance, would depend on their method of filing at the Home Office. Wherever letter sized containers are used, the size of the blank would have to be made to conform to such containers.

By using a heavy sterling ledger at the Penn Mutual Life Insurance Company, we have eliminated the necessity of filing our papers in envelopes. We would, therefore, recommend this type of paper to be used, inasmuch as it not only facilitates methods of filing, but it also facilitates the work of the examiner and enables him to present a better looking paper at the Home Office.

It seems advisable to have a blank of sufficient size, so as to have both Part II (questions asked by examiner) and Part III (physical examination) appear on the same sheet in order to keep the entire picture of the applicant in the mind of the Medical Director as he is reviewing the case; and also to facilitate checking at the Home Office.

Space should be conserved whenever a question is to be answered "no" or "yes" without further amplification. Whereas questions requiring explanations should be given additional space to enable the examiner to properly complete his report.

The question as to whether or not the application and the medical blank should be attached or separate is an open one. Our own recommendation is that they be kept separate. This expedites the work of the Agent in that he does not have to see that the application is in the hands of the examiner before going to the applicant, and it also obviates the necessity of the examiner having to turn over his report to the Agent in order to have the application completed before going to the Home Office.

It is hard to conform to the rule against allowing the Agent to see the medical blank when so many times he asks to have the application returned to him in order to make corrections, ask for additional insurance, etc. It also has a tendency to eliminate the practice of the Agent asking the examiner to take the application.

When it comes to the make-up of the questions it behooves us to get all the facts in the simplest manner. We should get the name, date of birth, residence, etc. Examiners should ask the occupation. In this day of occupational ratings it is essential that the true facts of a man's duties should be given to the Company in order that he can be properly classified. The family history should be elicited after the personal history is obtained. We have seen many applicants become so irritable under the strain of trying to remember the ages or cause of death of their grandparents or brothers and sisters, that they become disinterested in further questioning and render the rest of their answers valueless.

As regards the applicant's personal history we must remember that many of our present day examiners, and all our future examiners, will be hospital trained men and they will no doubt use the same methods in eliciting the personal history from an applicant for insurance as they do in obtaining a case history from a patient at the clinic. The quality of the examiner for life insurance has improved immensely in the last decade and will improve still more in the next as medical schools become centralized and medical education becomes standardized. The subject of examining for life insurance will be taught more and more. Students will not be taught, let us hope, to find out whether a man is insurable by reading to him seriatim all the names in the nomenclature. They will be taught rather to examine the applicant as an expert clinician takes his case history and makes his examinations in the amphitheater and in the laboratory. It is a question whether the companies which put the burden of proof on the examiner in a short, concise blank, do not get better results than those which attempt to assume it themselves by asking a multitude of questions to be answered "yes" or "no." It is absolutely certain that no company can get a good examination in that way from a poor doctor, and it only irritates an able examiner.

Questions in regard to the use of alcohol will have to be modified somewhat to conform to changed conditions brought about by the Prohibition Law. Most of the applicants reply

in a humorous vein that they drink all they can get, or suggest that the companies can stop asking that question now inasmuch as there is none to be had. As a matter of fact there is probably just as much if not more drinking going on to-day among men of the insurable class than there was before the days of the Volstead Act.

We have recommended two simple questions to cover this point.

First.—What is your daily consumption of alcoholic beverages?

Second.—Have you always been temperate in their use?

The question in regard to the use of drugs appears on a total of 47 medical blanks of different companies. While it therefore seems essential that this question be maintained on the uniform blank, we think it can be properly referred to in conjunction with the question regarding treatment for the alcoholic habit, reading as follows:

> "Have you ever taken treatment for the alcoholic or drug habit?"

This can be asked in conjunction with the first two questions regarding the use of liquors.

The question as to whether an applicant considers himself in good health at the time of the examination should be asked direct, even though the reply comes back, as it often does, "Well, if I were not in good health I would not be applying for life insurance." We do not believe that the companies can be asked to assume that this is the case. We consider it a very essential protective question from a legal standpoint as well as a good opening wedge for the examiner in obtaining a complete personal history. In conjunction with our next question, "When were you last attended by a physician," we have found it an excellent medium to refresh the applicant's memory. A careful examiner can amplify this question so as to ascertain, not only the last condition for which he was treated, whether it be a minor or a major illness, but also can cover practically

every condition for which the applicant has been under treatment for a good many years back. We recommend that these questions be put in the following way:

First.—Is your health impaired in any way?

Second.—When were you last attended by a physician or when did you last consult one?

By asking the name and address of the family physician we impress upon the applicant the importance of giving us the facts regarding his illnesses as he immediately assumes that it is our intention of checking up his statements by going back at the attending physician.

The question regarding rupture and the wearing of a truss has been simplified. We do not believe there is anything to be gained by asking an applicant to formally agree to wear a truss as long as he is insured, for no matter what his answer may be, the wearing of a truss will be largely a matter of personal convenience, and guided more or less by the opinion of his family physician. The description of the rupture should be noted in the physical examination.

After careful consideration of the various methods of eliciting a history of definite disease we have concluded to recommend the group method wherein diseases are classified under relative headings such as nervous diseases, respiratory diseases, etc. It seems to us that this covers the field thoroughly and efficiently without running through a whole list of separate diseases in a sing-song fashion that means nothing to the applicant.

The family history is kept intact with the exception of a few minor changes. We have eliminated the year of death as this is of little importance except, perhaps, in cases of tuberculosis which should be covered in answer to the question regarding tuberculosis in the family history. We have asked for the ages of grandparents but eliminated the cause of death.

Cancer, suicide, insanity, diabetes, Bright's disease, etc., would only be of importance if in the immediate family history (parents, brothers and sisters) and this information would be given in answer to the regular question regarding family

history. We have, therefore, eliminated these conditions and have modified this inquiry to refer to tuberculosis alone.

Now as regards Part III of the Application or The Special Report of the Medical Examiner.

The results of our comparison of the medical blanks in this respect gave us much less of an opportunity to use the pruning knife in order to render the blank uniform and concise. Practically all requirements are the same and our work in this instance has been therefore narrowed down to the simplest and most direct manner of obtaining the picture of the applicant and his exact physical condition. We have made no effort to shorten the work of the medical examiner in this part of the blank. Our experience has been that heretofore too much time was spent on the statements of the applicant to the detriment of the work of the examiner in making his physical examination. The examiners should be made to feel that, after all, the physical examination is the most important part of their work and that their chief responsibility to the companies they represent lies in an accurate and careful report of the physical condition of an applicant. We should ascertain how well the examiner may know the applicant, inasmuch as it gives us an opportunity to go back to him for additional information whenever we find they are acquainted, in re habits, etc. This is particularly of value in the smaller towns.

A careful inspection should be insisted upon and the examiner report as to appearance (is it healthy or otherwise) any deformities, rupture, etc. It is surprising the number of deformities that are overlooked, due to carelessness in their inspection, and which deformities are only brought out on our commercial reports.

Height and weight should be taken in ordinary clothing, requiring actual weight on scales wherever it is apparent to the examiner his applicant approaches either the over or under weight figure.

We have left out the question appearing in so many blanks under the caption of "Figure" (General figure). So many examiners seem to have difficulty in expressing themselves in

answer to this question. They contend they do not know just what the company wants. This condition should be properly covered if the examiner makes careful inspection report.

The "Shape of Chest" has also been eliminated for the reason that so many normal chests are irregular that an unfavorable answer to this question may create a wrong impresson at the Home Office.

We should require the pulse rate to be taken sitting. Our experience has been that the average pulse rate on an insurance examination runs well over the medical standard at 72. We think it is the practice of a great many examiners to allow some 8 to 10 beats due to excitement of examination. This practice can only be condoned if we are sure the examiner has eliminated the possibility of tachycardia, due to goitre. We have therefore included a direct goitre question for there can be no doubt that many are overlooked due to the fact that most examinations are made without having the applicant remove his collar.

A question referable to the condition of the teeth and gums has also been added because of the important bearing that diseased conditions of the mouth may have on systemic infections.

Blood pressure requirements should be made standard in all companies. It should be required in every case regardless of the amount of insurance or the age of the applicant. The auscultatory method should be used, the applicant placed in a sitting posture, and the readings taken on the left arm. Both the systolic and the diastolic reading should be reported.

Examinations of the heart should be made both standing and lying down. There is no doubt that a great many bad hearts get by because of the fact that the examiner has not listened to the heart in a prone position. It is, of course, impossible to insist upon this where examinations are made at a man's place of business but we think if examiners knew the companies made this requirement they would make more of an effort to complete their examinations in this manner. All

examinations of the heart and lungs should be made with the chest bared.

We have eliminated the temperature requirement because it is of little value to us unless we could have a series of afternoon readings. Any acute condition causing the temperature to rise should be easily apparent to the examiner without the benefit of the thermometer. We doubt whether it is actually taken when it is reported on the blank.

We should require that urinalyses be made under proper conditions. So many reports come from examiners that are made in dingy offices with artificial light where it is practically impossible to tell whether the urine is free from albumin or not.

We should standardize our specific gravity requirements and suggest that all companies adopt 1012–1030. The reaction of the urine has been omitted for the reason that it is rarely ever taken by the examiner in the field, and would only be of importance in those cases where specimens are sent to the Home Office, at which time this requirement could be properly checked up.

The female questions require no change. They are practically the same in all blanks.

The use of the voucher system is recommended.

Dr. Dwight—I am speaking for Dr. Toulmin who asked me to say that it is his hope that this matter of the standardization of medical examination blanks will be referred to a committee appointed by the chair, for present or future consideration.

Dr. Knight—Will you make a motion to that effect?

Dr. Dwight—I move that a Committee be appointed by the chair to consider fully this matter of the Standardization of Medical Examination Blanks, and report to the Association at a later date.

The motion was seconded by Dr. Dillard and carried.

Dr. Knight—Dr. Exton will now present "A Simple and Rapid Test for Albumin in the Urine."

A SIMPLE AND RAPID TEST FOR ALBUMIN

By Dr. William G. Exton

Director Prudential Laboratory, Newark, N. J.

Dr. Exton—Mr. President and Members of the Association—The desirability of a simple and rapid method for quantitating albumin in urine for clinical purposes has been expressed by many contributors to medical literature, and on taking up Home Office insurance work it struck me as a very strong probability that if we had a practicable and accurate method for the routine quantitation of albumin in urine it would prove extremely valuable for insurance work.

I need not tell you how defective all of our albuminuria statistics are. They not only lack information regarding amounts of albumin, but necessarily fail to relate such data to the specific gravity and volume of excreted urine. This last factor it is, of course, impossible to have in insurance practice and I may therefore refer to the ratio which I proposed in a previous paper as a substitute for this information in order to point out the possibility of refining and improving both statistics and selection by means of this ratio in conjunction with definite knowledge as to the quantities of albumin which may be present in given urines.

Anyone who seeks beyond the usual text books for closer information regarding the albumins in urine will not be otherwise than disappointed to learn how indefinite and incomplete is the actual state of our knowledge concerning them. Having this in mind as well as the many and perhaps insuperable difficulties which were likely to be encountered in working out such a method as we wanted, it was considered advisable to ground the work on some well-defined standard and this was determined by a critical comparison of the qualitative tests which have been proposed for clinical usage, particularly those which are in more or less general use. These comparisons were carried out on the same specimens of urine with

special attention to the action of the different reagents with reference to—1—Specificity as to albumin; 2—Reliability in showing albumin. As a result of these comparisons the Heat Test was adopted as a standard and proved all the more satisfactory for such purposes because the Heat Test, with or without brine is the standard usually employed in the larger clinical institutions.

Another preliminary to the work was a review of the literature which is surprisingly extensive, and which was undertaken with a view to helping us to decide as to the most promising angles of approach. Without going into all of the details involved in the many methods which have been proposed, and to some extent basing opinion on our own previous experiences with many of them, the sedimentation method appeared to us to be the most likely field in which to look for a quick and reliable technique. Although Folin and others, including the writer, had condemned such methods, as being wholly untrustworthy, the principle was exhaustively tried out with the reagents of Esbach and Tsuchiya and many modifications of these in connection with apparatus of special device working with vacuum filtration, etc. Failure to get reliable results, even with some improvements in manipulations and reagents, led us finally to abandon this field altogether and to try the principle of differential density which has been hitherto impracticable mainly because of the large quantities of urine needed for such estimations. With the development of the drop method of determining specific gravity by means of the writer's Immiscible Balance, a simple and rapid technique was worked out which has satisfied some workers.

Acidulate the urine with acetic acid (if desired add salt) and determine the specific gravity of a drop with the Immiscible Balance. Then pour a few cubic centimeters of urine into a test tube up to a fixed mark and boil. When the tube is cooled to room temperature add water up to the fixed mark on the test tube to replace that which was lost by evaporation. Filter out the coagulum and determine the specific gravity of a drop

of the liquid. The difference in specific gravity multiplied by Zuhor's Factor of 400 gives the amount of albumin in grammes to the liter.

When carefully performed the results obtained by this differential density method check, but it is not, however, a method which is suitable for indifferent technicians, and having in mind our own requirements and the large scale on which routine urinalysis is now done in many laboratories, it was thought worth while to try to work out a still simpler method.

The possibility of succeeding with some optical method was next investigated because such methods are usually precise and quick when the necessary instrument is not too complicated. It was learned that estimations of the albumin in urine may be made with the Refractometer, but fine adjustments and exact temperature controls are necessary, and the costliness of the instrument, taken together with other features were deemed to make refractometry, at least for the present, impractical for routine albumin work.

Studies were then made of Nephelometry and Turbidimetry. Experience with both of these methods soon leads to an understanding of the intricacies involved in the optics of turbid media, and without going into details it may be explained that up to the present time the cloudiness of liquids is measured in biological laboratories by what Wells calls "Intensity Methods."

These may be described as indirect or comparison methods, because a standard of known strength must be made up for the purposes of comparison with the unknown cloudy liquid; reflected light being used with the Nephelometer and transmitted light with the DuBoscq type Colorimeter. It is generally necessary to prepare several in order to have the standard solution correctly approximate the unknown solution, and the instrument must then be carefully adjusted by matching the standard against itself, after this the standard and unknown are compared and the value of the unknown is then computed by plotting a graph or by calculation.

These bare details will serve to show the impracticability of Nephelometry and Turbidimetry as at present done for such routine work as we have in mind, although the writer believes that either method will yield good results in the hands of a well-trained painstaking worker. In fact our observations impressed me strongly with the fact that the chief difficulties connected with Nephelometry and Turbidimetry are inherent in the suspensoids and their management rather than in the optical methods.

Further study of the properties of turbid liquids show that few indeed conform to the requirements of thermodynamic equilibrium as laid down by Tolman, and that the particles in suspension vary in size from the macroscopic to those of molecular dimensions. As Wells describes it "The smaller particles are in incessant Brownian motion. The smaller the particle the greater is the violence of this motion, so that small particles are continually coming into contact and coalescing, thus growing into relatively larger agglomerates and settling to the bottom unless the liquid is stirred mechanically or by convection currents."

From this it will be seen that a time factor is involved in the estimation of turbid liquids, and consideration of this and their other properties in conjunction with some of Tyndall's experiments and the peculiar complexities of the optics of Turbidimetry seemed to me to point to a promise of finding an improved method if it were possible to contrive some means of measuring directly the degree of cloudiness of the unknown solution.

In short it appeared to me that rapidity, simplicity and more uniform accuracy could be gained if one were enabled to dispense entirely with the standard solutions which are now used for comparison, *i. e.*, their preparation, manipulation, uncertainty of control, etc.

The familiar observation of objects beneath the surface of the ocean was, of course, common even in ancient times, and led to the invention of several Turbidimeters which give direct measurements by means of what Wells calls "The disappear-

ance method (as distinguished from the "Intensity Method"). These Turbidimeters follow two types, one for field work, the other for laboratory. The field type was designed by Hazen and in Leighton's improved form is now used for water and sewage analysis. Originally this consisted of a pin stuck into a piece of wood which was lowered into sewage until it vanished. Forty-five years ago Horning invented the diaphanometer which seems to have been the first laboratory apparatus to employ the "Disappearance Method" and this has since been improved by Parmelee and Ellms, Jackson, Leighton, Sheppard, Weaver, Smith and Parr and in one form or another they are, I believe, used principally for sulphur determinations, and it is worthy of mention that in 1911 Dawson and Basset described a method for estimating the number of bacteria in vaccines turbidimetrically with an arrangement connected with the microscope that combined the "Disappearance Method" with the use of standards for comparison.

None of the available instruments which work on the disappearance principle are even remotely adapted to our needs, because not only are impossibly large quantities of liquid required, but they are insensitive and uncertain, in short, too crude. In order to test out for our purposes the further possibilities of the "Disappearance Method" some experiments were made with cloudy urines viewed through a Colorimeter cup, like that supplied with the DuBoscq Colorimeter, but graduated so as to show the depth of the column of cloudy liquid through which a target was seen to appear or disappear. With varied illuminations many kinds of targets were tried, beginning with different specimens of fine print in black, gray, red and green. Then many and various geometrical designs were made up of lines and curves of several dimensions and degrees of heaviness in the same design, and these were etched on transparent glass, painted on opal glass and made of photographic film. Later narrow slits and pinholes were made by silvering or platinizing optical glass and these were used in several ways. The results of these many experiments with targets and illuminants showed that a distinct and reproducible

end or reading point was obtainable by utilizing the power of the column of cloudy liquid to obscure a design, or to extinguish light, or to resolve lines or slits placed closely together. With either of these optical principles it was possible for different observers to get readings with the same specimens which checked, providing only that the turbidity was not too slight.

Through the kindness of Mr. Karl Keuffel, of Messrs. Keuffel and Esser, of Hoboken, N. J., an instrument was constructed which consisted of a lamp enclosed in a light tight housing, on the roof of which was an aperture countersunk for the reception of both target and Colorimeter cup. Over and around this aperture was mounted an optical tube with plunger which is adjustable by rack and pinion in such a way as to show on a scale the height of the column of cloudy liquid in millimeters. Within its limitations the instrument proved satisfactory and many readings were made of urines which had been subjected to the Heat Test, but it was still necessary to use the word Trace for the specimens which were not measurable because of slightness of turbidity. In fact it was disappointingly apparent that with a decrease of turbidity the optical power of the cloudy liquid weakened logarithmically and that readings of slight turbidities would only be obtainable with an instrument of absurdly impractical length.

The coupling of this situation with our experimental verification of the almost obvious fact that there exists a close correspondence between the slightness of the turbidity it will measure and the fineness of the target that is used, led us to study the effects of different magnifications on targets of various degrees of fineness, such as micrometer rulings, etc.

These studies solved our optical problem, because it thus becomes possible not only to estimate extremely slight turbidities such as those which are not appreciable with the naked eye, but by reason of the magnification affecting the turbidity as well as the target a sharper reading or end point is also gained, and the view as seen through the instrument distinctly improved.

The instrument which I show you then has an eyepiece microscope which enlarges four times, and this had been found satisfactory for all useful ranges of turbidity which occur in connection with our tests for albumin in urine. Note the extreme simplicity and rapidity of manipulation. All one has to do is to pour the turbid liquid into the 100 millimeter high cup and place it in position in the light tight box. Rack down the plunger until the target is just visible and with a half turn or so cause the target to disappear. The vernier scale read in the convenient mirror tells in millimeters the depth of the column of cloudy liquid at which the standard target disappears. My technicians make these estimations in about a minute and they check within less than one millimeter.

It was originally intended to have the scale of the instrument show percentages of albumin as determined by our test in comparison with Kjeldahl's determination of the protein nitrogen multiplied by the 6.25 factor, and instruments with such scales will be available if they are wanted. In view, however, of the many applications of turbidimetry it was thought best to equip the instrument with a universal scale which fixes the depth at which the standard target disappears as a sort of unit or degree of turbidity measurement. It is not at all a difficult matter to construct and furnish tables which show the correspondence of percentages by a particular test with points on the turbidity scale, and for albumin in the urine such tables will probably be developed in relation to several tests. It is likewise worthy of mention that the instrument is also a self-standard Colorimeter, and with an appropriate light filter gives exact values of color intensity without the use of standards for comparison, and should, therefore, prove extremely convenient for Colorimetry.

Although we started our studies by adopting the Heat Test as a standard, it is a matter of common observation that the physical characteristics of the coagula which are obtained by boiling albuminous urine vary greatly depending upon the amount of albumin, the kind and character of salts in solution, the H-ion concentration of the urine, etc. This to some extent

introduces an element of change for which allowance should be made in making measurements of turbidities, and likewise for differences in the coarseness or fineness of the coagulum. Experiments were therefore made with many of the protein precipitants which have been proposed during the last few years in connection with the development of blood chemistry technique. In fact the whole gamut was carefully gone through and at the end we found ourselves in agreement with Folin that sulphosalicylic acid gave the most suitable precipitate with albumin in the urine for turbidimetry.

The literature discloses that sulphosalicylic acid has been employed since 1889 as a test for albumin in urine. It has been used in saturated solution as a layer test, and Dr. Sanford of the Mayo Clinic has lately stated: "We are strongly influenced to believe that the sulphosalicylic acid test should be considered standard." It has also been used by dropping a bit of the dry acid from a knife point into the urine and watching for a cloudiness. Anyone who compares the precipitate obtained with sulphosalicylic acid with that of the Heat Test will be immediately struck with the fineness of the sulphosalicylic acid precipitate. On this account observations were made with a view to taking advantage of this quality for turbidmetric purposes, and it was found that a one per cent. solution of sulphosalicylic acid added to an equal amount of urine would precipitate all of the albumin in those specimens of urine which contained maximum amounts of albumin; as a matter of fact the test proved to be an extremely rapid and trustworthy qualitative test, and if one shakes equal amounts of the sulphosalicylic acid solution and urine and waits for the maximum cloudiness to develop a quantitative albumin result is possible with the Turbidimeter in about the same time it takes to perform the Heat Test properly.

It was noted that certain urines take a little longer for the cloudiness to develop to the maximum than do other urines. These are generally specimens of low specific gravity which contain unusually large amounts of albumin, and in the effort to hasten the development of the cloudiness various reagents

were tried in conjunction with the sulphosalicylic acid solution. Of these, it seemed to hasten matters slightly to make up the sulphosalicylic acid solution with a four per cent. glacial acetic acid solution. With or without the glacial acetic acid I am of the opinion that the one per cent. sulphosalicylic acid solution used as I have indicated is a simpler and better qualitative test for albumin in the urine than is the Heat Test. In our Laboratory 6294 comparisons have been carefully made between the Heat Test with and without brine, Heller's Test and the sulphosalicylic acid test as we have developed it. The comparisons were made in this way. After a few hundred preliminary observations the technicians were told "Make these three tests on every specimen just as they come in and when you run into any discrepancy call me." During these tests I was called to see 18 discrepancies out of the 6294 comparisons. Of the discrepancies there were six cases in which the examiner had added Thymol as a preservative, and Thymol gives an imitation of the Heller's Test. In nine of the discrepancies an excess of Formalin had been added which also gives a false Heller's reaction, so that in 15 of the 18 discrepancies the sulphosalicylic acid solution proved to be the more reliable.

One discrepancy was unusually interesting because the microscopical examination showed 30 to 40 blood cells to the one-sixth field, and the sulphosalicylic acid test really did not show enough albumin to explain them. Further study of the specimen, checking up the findings by several tests led us to the conclusion that in that specimen we were not dealing with blood at all but with a particular spore that resembles blood cells so closely as to mislead the technicians, and the sulphosalicylic acid solution was proven right in that case.

Two other cases which gave definite reactions by the Heat and by Heller's Tests were found to be due to some resinous substances which dissolved on adding alcohol.

On the basis of these observations I am led to believe that our way of using sulphosalicylic acid is probably safer than the Heat Test. An extensive search of the literature has

been made and we find that practically all observers who have reported on the use of sulphosalicylic acid as a reagent for albumin in urine endorse its use. You know that it is the reagent employed in Folin's procedure. We are trying to load the solution so as to have its specific gravity approximate that of the albumin suspensoids and otherwise improve the test, but it is now a test which no one should fall down in using and which appears to be remarkably reliable.

It has also been learned by measuring turbidities before and after the precipitates have fallen that if the test tube be sharply shaken the particles in suspension return practically to their original state, and that is an immense advantage when it comes to doing turbidimetry.

Dr. Symonds—I would like to ask Dr. Exton if this solution does not precipitate nucleo-protein.

Dr. Exton—I think it does not, although I am not willing to be positive about that. If you want to avoid nucleo-protein in any test the thing to do is to add some acetic acid, and if you get any precipitate filter it out, and then go ahead and make your albumin test, or use the one per cent. sulphosalicylic acid without the glacial acetic acid. It means only a wait of about a minute more to get the end reaction.

Dr. Muhlberg—I should like to ask Dr. Exton whether the test works equally well with a turbid urine. We necessarily must work with turbid urines at times. Furthermore, have you checked up as to whether or not your acetic acid will not precipitate some of the urates. I should think in a specimen containing a great deal of urates that your acid solution would also precipitate some of the uric acid.

Dr. Exton—It is certainly safe as far as the urates and uric acid are concerned. We have checked it up and we have always made it a practice to get rid of whatever cloudiness we can. No matter what albumin test you use, you must try to do that, even though one cannot, of course, always do it. With this test I think you are in a better position than with any other, because you can estimate your turbidity before and after doing the albumin test.

Dr. Rogers—You get a reading for turbidity and then make your test and get another reading, and the difference is due to albumin?

Dr. Exton—Yes. That is right.

NOTE

Additional observations made since our paper was read show that with the exception of sulphosalicylic acid all of the acids in common use in the biological laboratory develop some cloudiness when added to pure solutions of nucleo-protein derived from yeast or fresh thymus glands (Hawk's technique). Albumin free urines to which the pure nucleo-protein solution is added in different proportions do not, however, develop a cloudiness regularly even with some dilutions of nitric or acetic acid.

As stated in the original paper I have been trying to improve the reagent by making the reaction as independent as possible of irregularities inherent in the varying salt content of urine, and otherwise by bettering the equilibrium of the turbid liquid, etc. From the beginning attempts were made to find a suitable loading for the reagent so that when mixed with urine the specific gravity of the mixture approximated the specific gravity of the albuminous precipitate. It has been found that crystalline sodium sulphate in a 40% solution with sulpho-salicylic acid gives to a great extent these advantages, and probably makes the reagent more specific for serum albumin and globulins than any of the albumin tests in ordinary use. The reagent will not develop any cloudiness at all, even when added to pure solutions of nucleo-protein nor with mucins. With egg albumin there is a great contrast between the marked cloud on heating and the faint cloud seen with the reagent.

By doing the test with progressive dilutions of a urine containing a large amount of albumin (1 to 2, to 4, to 8, to 16, to 32, to 64, etc.) the gradation of the albumin clouds may be prettily discerned with the unaided eye, and if the same amounts have been used for making the tests and the test tube

be allowed to stand over night, the precipitates which have fallen will be found to verify the dilutions. With the Turbidimeter, of course, finer and more exact distinctions are possible.

The improved reagent is made by adding to 40 grams of crystalline sodium sulphate (C. P.) enough water to make it up to 100 cc. and then adding two (2) grams of sulphosalicylic acid. The solution should be perfectly clear or filtered if necessary. The test is made by mixing equal parts of urine and reagent and allowing the test to stand for ten minutes before deciding as to the absence of albumin. Even if the precipitate has fallen, sharply shaking the test tube will render the liquid fit for the Turbidimeter.

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Dr. Knight—Dr. Dublin will present to us the work of Professor Dreyer of Oxford, in its relation to life insurance examinations.

THE WORK OF DREYER IN RELATION TO LIFE INSURANCE EXAMINATIONS[1]

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The subject "The Assessment of Physical Fitness" commends itself especially to an audience of Life Insurance Medical Directors. For, to assess physical fitness of applicants for insurance is exactly their function. One could hardly have hit upon a more apt title than has Dr. Georges Dreyer of Oxford for his recent book. When you pass upon an applicant for

[1] The writer desires to express his obligation to Dr. C. C. Grove and Miss Margaret E. Gantt of the Statistical Bureau of this Company for their valuable assistance in the preparation of this paper.

insurance, you assess his physical fitness on the basis of his structural and functional normality. You look for sound heart, lungs, and kidneys, for sound mind, and for evidence of normal health in the applicant's appearance and of his ability to carry on his daily work. More recently, you have turned to such supplementary evidence of physical fitness as that afforded by the interrelationship of height and weight according to age. There are, of course, other items, such as occupation, habits, the living conditions of the applicant, which assist in making up the judgment of what constitutes a standard risk. Your basic assumption is that every one of the points referred to has a definite and measurable effect on longevity and that in their totality, they give a fairly good guide for classifying applicants into those who will and those who will not measure up to the expectation of the mortality tables used as a basis for the calculation of premiums.

Dr. Dreyer is not concerned with life insurance examinations. He is interested primarily in developing a series of norms for distinguishing those who are physically fit from those who are not. These norms are based on the interrelationships existing between a limited number of bodily measurements, which are as follows: weight, height sitting or stem length, circumference of chest and vital capacity as measured by the spirometer. These measurements, which are easily made, Dreyer believes give a clue to physical fitness. He has discovered, he says, the true relationships which exist between these four measurements in the healthy individual. By actually applying the normal relationships to individual cases, he thinks he can distinguish those who are physically fit from those who are not.

Dreyer expresses the relationships between the four measurements referred to in a series of simple formulæ. Each one of these gives the value of one of the measurements in terms of one of the others. Thus, there are formulæ which express the relationship between the weight and the stem length, between the weight and the chest girth, between the chest girth and the stem length, and between each one of these three measures and the vital capacity. These six formulæ are applicable to per-

sons up to age 50. There is a set for males and for females, the two sexes being very different in the interrelationship of their measurements. To obviate calculations, the author in his book has appended a number of tables which express the values of these measurements in terms of each other. Thus, for each stem length in centimeters, a table gives the corresponding weight in kilograms; another table gives the weight corresponding to each chest girth. It is possible from the tables to read off what all the measurements of a normal individual should be from any one of the four that may be given. There are two sets of tables for both males and females, one in the metric system, the other in the American or English system of weights and measures.

The author presents a number of apt illustrations which show concretely the uses of his tables. The problem is to determine whether or not an individual is physically fit according to his actual measurements. Given the four measurements, he finds, first, the expected weight from the stem length and also from the chest measurement. If these agree with the actual weight, the person has normal proportions. But, in many instances, he finds normal persons whose theoretical weights according to stem length and according to chest girth are different. In those cases, he takes the average of the two theoretical weights and compares that average with the actual weight to determine the individual's fitness. If the individual has a normal weight, he then finds the theoretical vital capacity from the actual weight. He compares that figure with the actual vital capacity to determine the per cent. deviation from the theoretical vital capacity. If an individual has a weight which is 5 per cent. from the normal, he possibly has an abnormal weight; if he differs from the normal by as much as 10 per cent., he is probably abnormal with respect to weight; and if the deviation reaches as much as 15 per cent., his weight is certainly abnormal. Individuals may be abnormal as to weight and still be physically fit. If they are overweight and their vital capacity is commensurate with their added weight, they are considered physically fit because they have a larger vital

capacity than would be expected from their stem length and chest. On the other hand, persons who are underweight for their stem length and chest, may be physically fit if their vital capacity is commensurate with their stem length and chest. The following are two illustrations, the one taken from Dreyer's examples of a person normal in all respects, the other improvised to illustrate a person who is physically unfit according to the Dreyer standards.

ILLUSTRATION 1[1]

Female, Age 21 Years

Observations	Weight of body	= 62.0 kilograms
	Length of trunk	= 88.8 centimeters
	Circumference of chest	= 75.0 centimeters
	Vital capacity	= 3630 cubic centimeters

Calculation

Weight derived from length of trunk (88.8 cm.) = 64.71 kilos (from Table VII.).

Weight derived from circumference of chest (75.0 cm.) = 59.16 kilos (from Table VIII.).

Averaging $\frac{64.71 + 59.16}{2}$ = 61.9 kilos, which is the normal weight corresponding to the observed length of trunk and circumference of chest.

Subtracting the calculated weight from the observed weight,

$$62.0 - 61.9 = +0.1 \text{ kilos}$$

$$\frac{+0.1 \times 100}{61.9} = +0.2\% \text{ (or roughly } 0\%)$$

Therefore the person weighs exactly what she should weigh according to the Tables.

[1] Taken from p. 109 of *The Assessment of Physical Fitness*, by Georges Dreyer and George F. Hanson. American edition, Paul B. Hoeber, New York, 1921.

Since by the above calculation the person's weight is found to be normal, the vital capacity as calculated from the observed weight will be correct; therefore:

Vital capacity derived from weight of the body (62.0 kilos) = 3556 cc. (from Table X., Class A).

Subtracting the calculated from the observed vital capacity,

$$3630 - 3556 = +74 \text{ cc.}$$
$$\frac{+74 \times 100}{3556} = +2.08\% \text{ (or roughly } +2\%)$$

Therefore the person has 2% greater vital capacity than she should have according to the Tables for Class A, calculated from the weight of the body.

ILLUSTRATION 2

Male, Age 25 Years

Observations	Weight of body	=	121 lb. 3 oz.
	Length of trunk	=	$34\frac{1}{4}$ inches
	Circumference of chest	=	$33\frac{1}{2}$ "
	Vital capacity	=	3272 cc.

Calculation

Weight from stem length	=	135.35 lb.
" " circumference of chest	=	137.99 lb.
Averaging above weights	=	136.67 lb.

Therefore the person is 11.3 per cent. underweight.

The vital capacity if obtained from the actual weight would be too low for the stem length and chest girth. We, therefore, calculate the vital capacity from the stem length and circumference of chest as follows:

Vital capacity from stem length	=	4061 cc.
" " " chest circumference	=	4118 cc.

The average = 4090 cc.

The actual vital capacity is 20 per cent. below normal by stem length and chest circumference.

This individual is, therefore, not physically fit according to Dreyer.

The two illustrations above show how Dreyer's tables may be used to discover individuals who are physically fit or abnormal according to his standards. He is particularly convinced of the value of the vital capacity as an index of physique and health. By applying his values for vital capacity to the measurements of a large number of tuberculous patients, he has been able in the great majority of instances to determine the stage of tuberculosis, to give the prognosis in these cases, and to indicate the benefit derived from sanatorium treatment. He believes that his measures disclose the existence not only of tuberculosis but of other diseases as well. This phase of the work has been confirmed by a number of other observers for cases of tuberculosis and of heart disease.

Obviously, a work which promises to do all of this should be of the greatest interest to life insurance executives. For, if we can through a series of simple measures really distinguish the fit from the unfit, we have made an important advance. But, before we can accept as demonstrated so epoch-making a contribution, we must be very careful that the premises are correct and that the conclusions are sound. Let us then see how far justified the conclusions of Dreyer are from the internal evidence presented, and to what extent the conclusions can be applied to the life insurance business.

To evaluate the work of Dreyer, it is necessary not only to read his book, but to study very carefully a number of highly technical papers which he has issued from time to time. The most important of these appeared in the *Lancet*, August 9, 1919, and was entitled, "The Normal Vital Capacity in Man and Its Relation to the Size of the Body." In this paper, the author derives the various formulæ which express the relationship existing between the four measurements already referred to. The relationships are worked out from the measurements of sixteen males who ranged in age from 13 to 52 years and were selected for their physical fitness.

Strangely enough, Dreyer does not use the same formulæ in his book as in his *Lancet* paper. The following parallel columns will show the differences:

Comparison of Formulæ for Various Measurements for Males[1]

	BOOK	LANCET PAPER
1.	$W = .38025 \sqrt[.319]{\lambda}$	$\lambda = \frac{W^{1/3}}{.449}$ or $W = .0905 \sqrt[.33]{\lambda}$
2.	$W = .662 \sqrt[.365]{Ch}$	$Ch = \frac{W^{1/3}}{.457}$ or $W = .0954 \sqrt[.33]{Ch}$
3.	$Ch = \frac{\lambda^{1.1442}}{2.00148}$	not given
4.	$V.C. = \frac{W^{.72}}{.69}$	$V.C. = \frac{W^{.72}}{.69}$
5.	$V.C. = \frac{\lambda^{2.257}}{6.1172}$	$V.C. = \frac{\lambda^{2}}{1.9}$
6.	$V.C. = \frac{Ch^{1.973}}{1.5595}$	$V.C. = \frac{Ch^{2}}{1.82}$

Formulæ one and two for males as given in the book are very unlike those in the *Lancet* paper. They are, moreover, not the ones from which the tables in the book were computed. As might be expected, they are not consistent with the other formulæ. Number one should read $W = .0483 \sqrt[.319]{\lambda}$ and number two should read $W = .3229 \sqrt[.365]{Ch}$. With these corrections, the six formulæ of the two series agree very closely even if they do not appear to on inspection. There are similar discrepancies in the formulæ presented for females. Instead of $W = .36093 \sqrt[.313]{\lambda}$ the formulæ should read $W = .03856 \sqrt[.313]{\lambda}$ and $W = .30213 \sqrt[.284]{Ch}$ should be $.01478 \sqrt[.284]{Ch}$. Otherwise, the formulæ are mutually consistent and are used in compiling the tables. It is difficult to understand how these errors have crept into the book. But, they are there.

Let us now consider the set of formulæ as corrected. Do they truly express the relationship between the various measurements that we are considering? To answer this question, we attempted to derive the formulæ independently. By the method of least squares, we obtained a series of values which, while they fitted the facts pretty well, did not present as close a fit as the values obtained by Dreyer. We did not exactly

[1] W = weight in grammes; λ = stem length in centimeters; Ch = chest circumference in centimeters; V. C. = vital capacity in cubic centimeters.

duplicate any of the Dreyer formulæ, although we could approximate a number of them on theoretical grounds. It must be at once admitted that his formulæ fit the data for the sixteen males better than any other values we could obtain. Let us examine some of these formulæ separately.

$$\text{V. C.} = \frac{W^{0.72}}{0.69}$$

This formula is the most important in the series. On applying it to each one of the sixteen individuals, we found only one whose actual vital capacity deviated considerably (+ 6.7%) from that predicted by the formula. But, in view of the fact that an excess of vital capacity is a favorable feature, no fault can be found with the fit of the formula to these individuals. Similarly, Dreyer applied this formula to the material of Schuster and he shows that the relationship between weight and vital capacity in this large group of Oxford undergraduates as a whole is in accordance with the formula.

We have in like manner attempted to apply this Dreyer formula to a group of Harvard undergraduates[1] and have found that these boys had with few exceptions a higher vital capacity than that predicted by the formula. This finding does not in itself impair the value of the formula because as Dreyer says the vital capacities given are averages for fit individuals and may be exceeded by highly developed and athletic persons.

His other formulæ are not quite so fortunate. Let us take the one expressing vital capacity in terms of stem length. The formula is:

$$\text{V. C.} = \frac{\lambda^2}{1.9} \text{ or } \frac{\lambda^{2.257}}{6.1172}$$

When applied to his sixteen individuals, we find that one person, No. 12, has a vital capacity which is 20 per cent. in

[1] West, "Clinical Studies on the Respiration," *Arch. of Int. Med.*, vol. xxv., p. 306.

excess of that predicted; whereas, No. 16 is 13 per cent. below. There are altogether six individuals whose vital capacity is at least 7.85 per cent. above or below the normal. If we use the second or more accurate formula which he used in his book, we find that there are five individuals whose vital capacity is at least six per cent. above or below the expected. These are presumably fit people and since there are two individuals out of 16 who are seriously below the expectation, it would seem that either there is something wrong with the formula as an expression of fitness or that there are wide limits between which fit individuals can be found. The above deviations from normal vital capacity are almost as marked as are those calculated from height standing according to Hutchinson's method which Dreyer insists has no value. Still he puts much store by this expression for vital capacity and height sitting and prefers it even to the previous formula showing vital capacity in relation to weight—because, as he points out, weight may change rapidly but stem length does not.

Likewise his formula for vital capacity and chest,

$$\text{V. C.} = \frac{\text{Ch}^2}{1.82} \text{ or } \frac{\text{Ch}^{1.973}}{1.5595}$$

shows a considerable range of deviation for his sixteen individuals. There are five persons in his series who are all the way from six to ten per cent. below the expected vital capacity. Yet he says that the vital capacity of fit persons does not deviate as much as ten per cent. below the normal. Following out his own procedure, we averaged the calculated vital capacities as obtained from both stem length and from chest. We found by this method that of the sixteen persons chosen specially for their physical fitness, three have a vital capacity seven per cent. or more below the normal. One of these, No. 16, is 8.8 per cent. below, which is close to the limit for "probably" unfit individuals by Dreyer's standards. This same individual is 7.7 per cent. underweight for his stem length and chest girth and 3.1 per cent. under vital capacity even for his underweight.

There is another line of evidence that all is not well with these formulæ. Take, for example, the relation between weight and stem length and weight and chest. Dreyer, equating the two formulæ, concludes that "in the normal healthy man the circumference of the chest is, on the average, about 1.5 per cent. smaller than the stem length, though in a fair number of individual cases the chest measurement may be greater than the stem length." On applying this test to his own series we find that four out of the sixteen individuals have chest girths 4.5 to 6.2 per cent. greater than their stem length and four more whose chest girth is from 5.5 to 10.1 per cent. less than their stem length. There is, obviously, too much variation to satisfy the rigid expressions laid down by Dreyer. The lesson from these several discrepancies is that Dreyer attaches more value to his formulæ than is justified by the facts.

We have in our previous discussion referred to the formulæ for males only. One-half of Dreyer's book is given over to tables for females which show the relationships existing between the four measurements heretofore discussed. These tables are constructed on the basis of formulæ similar to those for males. But, nowhere does Dreyer give us any clue as to how these formulæ were obtained, how many lives they were based upon and whether they have been confirmed by others. In fact, they are interjected into the book without any explanation. I have already called your attention to errors in the two basic formulæ for weight in terms of stem length and chest. But, even a very cursory examination of the tables and the formulæ shows that they are inconsistent with some of the well known facts with reference to the relation of build of women to that of men. For example, Dreyer's tables show that for the same stem length or height sitting, women are heavier than men. For a height sitting of 35 inches, a male according to Dreyer should weigh 137 pounds and a female, 143 pounds, that is, a female is 5 per cent. heavier. But, we know that for the same height standing, females are a little lighter than males. The anthropologists tell us that females have a relatively greater height sitting than males have. Therefore, it

would follow that for the same height standing, females would be lighter according to our standard tables of height and weight whereas, they would have longer stem lengths and, therefore, appreciably greater weight than the males according to the Dreyer tables. Which is correct? The discrepancy is really very considerable, and I am inclined to say that, for the present at least, no use should be made of that section of the Dreyer book which relates to females.

The important question for us to-day is what use we can make of the Dreyer work. I am frank to say that it is as yet very limited. The Dreyer researches are still in an experimental stage. We should hesitate to apply standards to practical business affairs, some of which are very likely to be much changed as they are derived from the measurements of larger groups of people whose physical condition is more definitely known than those reported upon by Dreyer.

But, quite apart from the accuracy of the formulæ, there is the fundamental question as to their significance. Dreyer insists that they express the relationship between the various measurements in physically fit people and that they can be used to distinguish between those that are physically fit and those that are not. Unfortunately, this is the weakest spot in the entire work. For, what is meant by "physical fitness?" If this is an expression merely of the general vigor of an individual or of individuals as determined by their appearance, then, obviously, there is little value for us in the characterization. We know only too well that individuals of good general appearance may be found on careful examination to be suffering from serious physical defects. If we rather assume that the sixteen persons who made up Dreyer's series and the other groups which also confirm his findings were persons who on actual physical examination were found to be free from organic disease, then we have a better basis. But, even then, the applicability of the work to insurance must be limited. For, in the last analysis, the insurance examiner is concerned with fitness in the sense of the ability to live. This is the one test of physical fitness which insurance constantly has in mind.

In this sense, physical vigor is not the only consideration. Personal history, habits, and a host of other items enter as determinants of longevity. "Physical fitness" and "longevity" are far from synonymous. We know that certain races are notoriously long lived, but are not physically well developed; that females have greater longevity but poorer physique than males and that certain occupations, especially where the work is sedentary, lead to the development of poor physique, but do not necessarily involve a short life span for the workers.

It must be clear, therefore, that, from the insurance standpoint, the Dreyer work is at its inception. What is necessary for us is a series of measures of the Dreyer type which is definitely known to express relationships among individuals who correspond in insurance language to standard lives. We must avoid loose or vague expressions. By standard lives, as opposed to the physically fit we mean individuals who will give a subsequent mortality corresponding to our usual tables. With reference to Dreyer's fit persons, we can say nothing as yet. The problem then resolves itself into tracing the subsequent mortality of various groups of individuals whose relative indices are known. We shall in that way build up a body of information which is likely to be very useful to us.

Yet, the work is very suggestive, and it would be entirely unfair to leave the impression that there was no utility for us in the Dreyer studies. He has opened up a wide field for further investigation. The most fruitful contribution for insurance purposes is likely to come out of a revision of the indices giving the relationship between height sitting, chest girth and weight. We shall have to forego the idea of perfect correlation between these three measurements and attempt rather to find the limits of their variability among standard lives. A series of tables should be prepared showing, for example, the distribution of weights for each inch of height sitting and the mortality experience of each combination. A similar table for chest girth and weight would be called for.

Research might show that the best results for insurance work follow from a combination of chest girth and height sitting in relation to weight. Such tables will agree better with the known facts of human variability than do the rigid formulæ of Dreyer which leave, theoretically at least, little or no room for variation from the type.

The height and weight tables we now use in insurance work illustrate the other extreme, that is, of too much variability of weight with reference to total height. For, as you know, the weights for any given height may vary enormously from the average. In height and weight relationships, there is a very wide range instead of concentration at the average. Furthermore, mortality is apparently not seriously affected by this extreme range of variability; for individuals may be as much as 30 or even 35 per cent. below the average and still enjoy standard mortality. For underweights at least, our height and weight tables have apparently little value. The coefficient of correlation between total height, weight and mortality is too low to give useful indices for insurance purposes.

It is because of this defect in the height and weight tables that the alternative measurements of Dreyer for height sitting, chest girth and weight may prove of the greatest value. These three measurements are much more closely correlated than are height standing and weight. The limits of variability will be found much less. It is necessary only to identify the various combinations with the facts of mortality. This has not yet been done. It is the chief suggestion that arises out of the consideration of the Dreyer work. For, it is possible that when further investigation is carried out, that we shall have guides as to build which will be ever so much better than the height and weight relationships we now use.

The relationship of vital capacity to weight, while probably the most accurate in the whole series presented by Dreyer, is one which unfortunately is not adapted to the insurance business on a large scale. Spirometer readings can be very much modified through exercise, and it would be entirely possible for an applicant with a low spirometer reading to increase his

vital capacity very appreciably through exercise and practice without materially improving his health. Spirometers are rarely, if ever, found in physicians' offices and the use of this rather expensive instrument must be limited to laboratories. It is conceivable however that the method may be useful in clearing up some doubtful cases where the build of the applicant suggests defects but which are not proved on the examination. Take, for example, an applicant who is overweight 30 per cent. and who otherwise presents a good picture of physical health. According to present practice, we should be suspicious of him especially if the amount of insurance were considerable. Our practice would be to reject him or to offer him substandard plans. But, suppose the individual has the predicted weight for his stem length and chest measure and the normal vital capacity for his weight. It is probable that such a person would prove a much better risk than the applicant who was overweight for his other proportions and whose vital capacity was too low for his extra weight.

It is possible, however, that the relationships between vital capacity and the other measurements may prove useful in singling out cases of incipient tuberculosis. Dreyer's work seems to indicate that there is a much lower vital capacity among persons in that condition. The Dreyer measures might serve as one more guide in forming a judgment on such applicants. In the same way, the Dreyer measures for vital capacity when applied to cases of arrested tuberculosis might very well indicate which were the safest risks.

But, the chief value to us in the work of Dreyer lies in the future. He has opened up an obviously rich field for further investigation. It may well be that the insurance companies are the best agencies for revising his formulæ and for adding to their value. For such units as they will prepare will measure the very definite character of longevity rather than the vague condition of physical fitness. I am inclined to believe that many contributions will be made at the future meetings of this Association by members engaged in revising and refining the very fertile work of our author.

DISCUSSION

Dr. Knight—Dr. Eugene L. Fisk, of the Life Extension Institute has very kindly consented to come here to-day to discuss this paper of Dr. Dublin's. I take pleasure in introducing Dr. Fisk, who, however, is known to most of you.

Dr. Fisk—This Association has made a very valuable contribution not only to life insurance medicine but to clinical medicine in presenting this study of Professor Dreyer's method. Dr. Dublin, with his usual penetration and thoroughness, has applied to this method the criteria employed in the consideration of large masses of lives, thus supplementing and testing the evidence derived from the more individualistic work of the clinician. He has clearly pointed out the weak spots in the mathematical structure of the theory but refrains from tearing it down. If in discussing this paper I venture to cover some of the same ground so admirably covered by Dr. Dublin, it is to present the reaction of the medical man in terms of office practice to the weak and strong points of the method.

It is important to examine this method (first) as to its clinical value; and (second) as to its value in life insurance, hygiene, and preventive medicine as a means of determining physical fitness. I heartily agree with Dr. Dublin in raising a question as to Professor Dreyer's use of the term "physical fitness." Does he mean, as it is practically asserted in the introduction to his treatise, fundamental physical fitness, underlying resistance to disease—in a word, longevity—or does he mean present physical fitness or well being? I am aware that clinicians often err in confusing immediate physical fitness or a condition in which the organs are in balance and functioning up to the normal, with fundamental physical fitness or a physical state free from underlying physical defect or lowered resistance to disease. This confusion of ideas with regard to physical fitness is one of the greatest obstacles to the progress of personal hygiene and physical upbuilding of the nation's health and we know that it is the first lesson that those entering the field of medical selection for life insurance *must* learn.

We can readily think of many conditions that would not affect the vital capacity in people originally well developed, for example, gallstones, incipient kidney trouble, focal infection in its initial stages, even latent syphilis. There is nothing in the Dreyer studies to show that the vital capacity in a wide range of possible substandard conditions which experience has demonstrated affect physical fitness and longevity, is materially reduced. There is a certain contradiction in his statement on the one hand that these tests are measures of fundamental physical fitness, and on the other hand, that they bear no necessary relationship to physical defects.

Putting this aside for the moment, we can perhaps do better justice to this method if we first examine it on the positive side. From a study of the literature and some experience with the method in the office of the Life Extension Institute, I think it may be fairly stated that vital capacity tests have a high clinical value, especially in marking the clinical progress of a patient. Also I think we may accept the fact that a very low vital capacity reading indicates a substandard condition of health. This, however, according to Dreyer's own findings, may be a temporary condition. He plainly states that the vital capacity is materially influenced by activity and environment and that it is not necessarily a reflection of fundamental condition or type. When we examine the values of normal or supernormal vital capacity readings as evidence of physical fitness we are, as Dr. Dublin points out, by no means on such secure ground, taking Professor Dreyer's evidence as it comes. In his studies at the Brompton Hospital of 200 cases, he emphasizes the fact that 17 of the hospital staff known to be normal showed normal vital capacity; but he ignores the fact that out of 116 cases known to be pathological, 30% showed vital capacity within the normal range, that is, either slightly above the normals he has fixed or less than 10% below. It is clearly apparent from his studies that a subject with active, though very much improved, tuberculosis can register a normal vital capacity. Of course the clinical attitude of mind toward such a case is extremely favorable—an expectation of a permanent cure is

justifiable—but we know from mortality experience on such cases that the risk is a substandard one.

Peabody and Wentworth's studies at Peter Bent Brigham Hospital give further evidence of the clinical value of vital capacity readings, especially in heart disease. Here again, however, we meet the limitations of this test. Lowered vital capacity readings are apparently only found in those cases where there are secondary signs such as dyspnea or a failure of compensation. For example, 25 cardiac cases with dyspnea showed 90% or better vital capacity. One of these had a low grade endocarditis. Forty-one cardiac cases, with a history of dyspnea, showed 70 to 90% vital capacity. One cardiorenal case (since died) showed 76%. One double mitral and auricular fibrillation, at work one year after observation, showed 74%. A cardiorenal case, 85%. A group showing 40 to 70% vital capacity presented severe advanced conditions with dyspnea. Another group, bed-ridden, showed 40 to 45%. We see here a very close agreement between vital capacity readings and the general physical state. Nevertheless it is evident that this is a partial functional test and subject to the limitations of all functional tests that we employ, such as renal efficiency, blood pressure, blood chemistry, and the like. Normal readings do not necessarily exclude the possibility of fairly well advanced organic change or fundamental physical insufficiency that would justify placing the individual in a high mortality class.

It is true that Peabody and Wentworth's normal standards were rather roughly fixed and based upon height, as follows:

Men		Women	
6′	5100	Over 5′ 6″	3275
Over 5′ 8½″ to 6′	4800	Over 5′ 4″ to 5′ 6″	3050
5′ 3″ to 5′ 8½″	4000	5′ 4″ or less	3825

According to our observations, however, the normals are high for the class of people examined and the general testimony squares with that derived from Dreyer's studies at the Brompton Hospital.

These clinical observations lead us to the conclusion that from the standpoint of life insurance medicine and the "assessment of physical fitness," to use Dreyer's own term, the test has a distinct value; but, also, very distinct limitations. In other words, it is a test which may oftentimes contribute valuable information as to the physical future of an individual, when considered in relation to the family and personal history and other evidence derived from a general physical examination. There is also a possibility that by taking the weight in relation to the stem height, individuals may be more accurately classified. We have already noted individuals, apparently physically fit except for 20 to 40 pounds underweight according to the standard tables, who were exactly normal in weight according to the Dreyer tables, and we know what a low mortality was found in the medico-actuarial investigation among well selected, extremely underweight types. An extreme instance of the unreliability of total height and body weight as indices of physical condition is the gorilla who measures 5 feet 1, and weighs 418 pounds. With legs proportionate to the body as in man, he would measure 7½ feet and weigh over 500 pounds. Recent studies by Gray and Walker confirm the Dreyer weight standard.

This discussion would not be complete without reference to other tests of respiratory efficiency. The *Journal of the American Medical Association*, in severely criticizing the Dreyer methods, rightly insists upon the fact that total respiratory efficiency cannot be determined by merely measuring the volume of air respired, as efficiency involves not only chest volume but circulatory, blood, and tissue conditions. It seems important, therefore, to consider the Flack tests, which include breath-holding, expiratory force tests, and expiratory fatigue tests. These were employed by Flack in the examination of candidates for the Royal Flying Corps and in checking up the condition of flyers. In the breath-holding test the subject expires deeply, then fills the lungs fully, and with the nose clipped holds the breath as long as possible. In the expiratory force test, the subject blows a column of mercury in a U tube

as high as possible, the result being recorded as in blood pressure by millimeters of mercury. In the fatigue test, the subject blows the mercury after full inspiration to a height of 40 millimeters and holds it there without breathing as long as possible.

In studying the results of these tests I have been impressed by the fact that the expiratory force test and fatigue test are more reliable in revealing fundamental physical unfitness than the vital capacity tests and are quite as consistent in their testimony as to physical fitness. It should be remembered that both the vital capacity and the Flack tests show a high possible range above normal, depending upon the individual, his activities, and the original physical endowment, as well as present condition of health. The following table from Flack's report will make clear the more positive significance of the U tube tests:

FIT GROUPS

	Vital Capacity	Breath-Holding Test	Exp. Force Test	Fatigue Test
(1) Fit flight commanders, etc.	4062	67	112	52
(2) Fit home defense pilots	3940	72	119	50
(3) Cadets	3823	69	106	51
(4) U. S. Cadets	3814	66	116	53.5
(5) Delivery and test pilots	3620	57	108	40
Average	3852	66	112	49
UNFIT GROUPS				
(1) Pilots taken off, showing stress	3480	49	74	25
(2) Hospital cases	3560	54	87	35.5
Average	3520	51	81	30

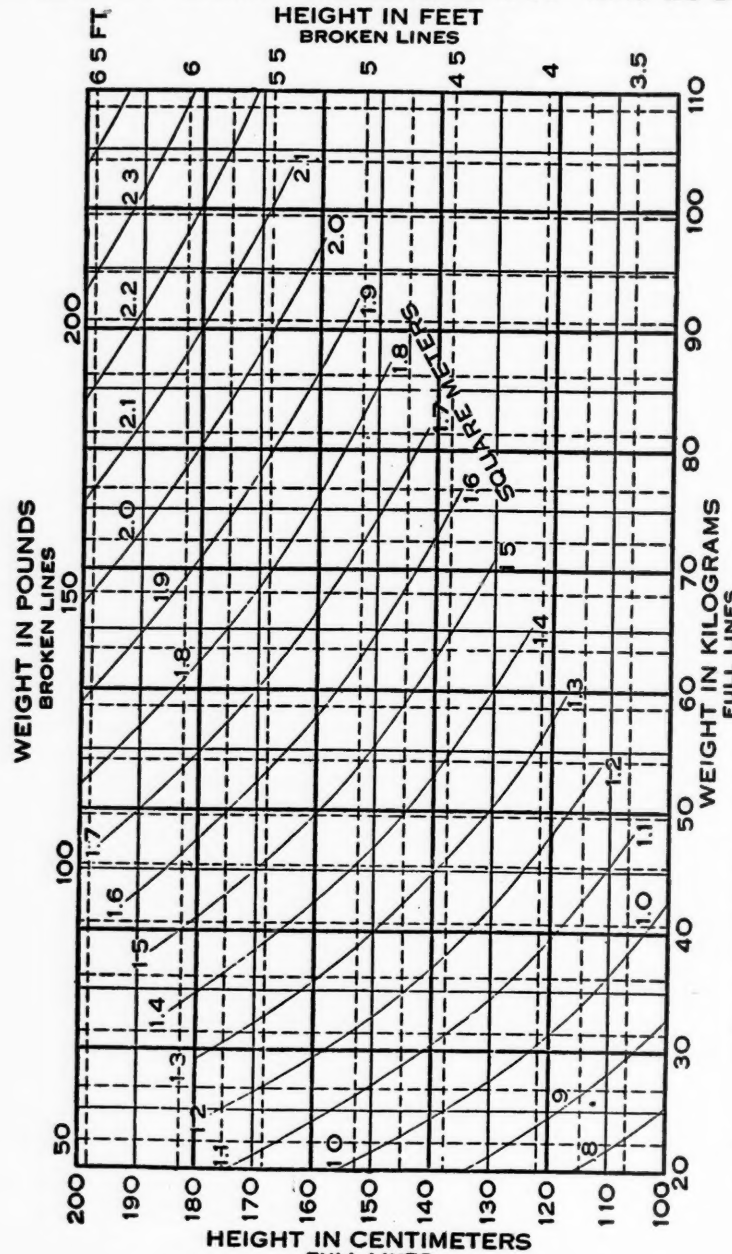
Note that in unfit classes the average drop in vital capacity was less than 10% below the average vital capacity of the fit classes. But the drop in the expiratory force was 29%; in breath-holding 22%; and in the fatigue test 40%. I am inclined to believe that in these U tube tests we have a more

convenient, and perhaps more consistent method of determining the total physical condition of the individual. It should be remembered that the expiratory force test does not measure the vital capacity or volume of air that can be forcibly expired over and above the residual air. It is a measurement of muscular and nervous tone. Theoretically it should be of value in early tuberculosis if, as Lundsgaard and Van Slyke claim, there is in this condition an increase of residual air and reduced vital capacity due to impaired movement of the diaphragm and chest walls. Also we would expect *a priori* that a reduced physical state would impair the power of the chest walls and lower the endurance as shown in the low ratings derived from the breath-holding and fatigue tests in unfit subjects. If these Flack tests prove of high practical value, they can be readily combined with blood pressure observation and perhaps some relationship between blood pressure changes and the Flack ratings established, as well as between the Flack ratings and the vital capacity.

Heald and Thompson have endeavored to derive a formula that will combine the Flack and the Dreyer tests, but this is as yet in an experimental stage.

As to the practical use of the Dreyer tables in clinical and life insurance work, this presents many difficulties but they are not insuperable. In both clinical and life insurance work, it is desirable to reduce the method to the simplest form compatible with accuracy, and the question of apparatus must also be considered if the test is to be widely employed. If the subject is of normal weight, it is a simple matter to find the vital capacity reading in the Dreyer table and judge the individual according to his class. Many people will depart materially from the normal weight and then the procedure is more complex. In a study of the work done at various centers, I am impressed by the value of West's investigations at the Peter Bent Brigham Hospital and Harvard Medical School. Following out Dreyer's own suggestion that the vital capacity is a function of the surface area of the body, he has developed a simple arithmetical formula that can be applied to the surface

BODY SURFACE CHART (DRS DU BOIS)



Plotted from the Du Bois new formula for Body Surface which is: Area in square centimeters equals Constant 71.84 times Weight of person in kilograms raised to the 0 425 power, times Height in centimeters raised to the 0.725 power. See *Archives of Internal Medicine.* June, 1916. Vol. 17, part 2, pp. 863-871.

area of the body. The surface area can be readily ascertained from the graphic chart of Dubois and Dubois, presented herewith, which has been adapted for practical clinical use with the apparatus for determining basal metabolism.

This graphic chart is very similar to Dr. Rogers' weight chart. With the net weight and height known, the body surface in square meters can be ascertained from the chart and this factor multiplied by 2.5 liters gives the normal vital capacity for men; and multiplied by 2 liters gives the normal vital capacity for women. I have tested this formula for various heights and weights and find that the results closely approximate the Dreyer standards of Class A for men. I have found that Class B is 8½% less than Class A; Class C 14½% less than Class A. Women as a class are 13% below men in vital capacity in the Dreyer tables, and the difference in the classes is in the same ratio as among men. These figures give us short-cuts to computing normal vital capacity and the departures therefrom shown by those tested. In using the body surface method, the class of the individual as determined by his actual occupation and environment should be fixed and then his actual vital capacity compared with the standard for his class as determined by the factors given. He may, of course, finally be placed in a lower or higher class than his occupation and environment indicate. Professor Dreyer's rough classification of occupation does not square with life insurance mortality experience yet may be found accurate from vital capacity standpoint.

I also submit a table I have worked out showing among males the cubic centimeters of vital capacity per pound for all weight groups. For people of normal weight this table can be readily used together with the other factors I have already quoted for determining the class and the vital capacity rates for women. As Dr. Dublin points out the vital capacity as based upon weight is more accurate than that based upon height, but the body surface and height seem by far the simplest and most direct method.

MEN

Pounds		*Cubic Centimeters*						
40 to 44	×	42	= Vital	Capacity,	Class	A.	(Class B = 8½% less; Class C = 14½% less than Class A.)
45 to 49	×	41	=	"	"	"	"	"
50 to 54	×	40	=	"	"	"	"	"
55 to 59	×	38	=	"	"	"	"	"
60 to 64	×	37	=	"	"	"	"	"
65 to 74	×	36	=	"	"	"	"	"
75 to 79	×	35	=	"	"	"	"	"
80 to 89	×	34	=	"	"	"	"	"
90 to 99	×	33	=	"	"	"	"	"
100 to 114	×	32	=	"	"	"	"	"
115 to 124	×	31	=	"	"	"	"	"
125 to 144	×	30	=	"	"	"	"	"
145 to 159	×	29	=	"	"	"	"	"
160 to 189	×	28	=	"	"	"	"	"
190 to 209	×	27	=	"	"	"	"	"
210 to 219	×	26½	=	"	"	"	"	"
220 to 234	×	26	=	"	"	"	"	"
235 to 249	×	25½	=	"	"	"	"	"
250 to 269	×	25	=	"	"	"	"	"
270 to 289	×	24½	=	"	"	"	"	"
290 to 299	×	24	=	"	"	"	"	"

WOMEN

Class A—13 % less than Class A for men
Class B—8 % less than Class A for women
Class C—14½% less than Class A for women

Therefore the following formula may be applied:

V. C. Class A (Men) × .915 – V. C. Class B (Men)
V. C. Class A (Men) × .855 – V. C. Class C (Men)

As the net weight is not readily secured in life insurance examinations and estimates of clothing weight may be very inaccurate, the following table from "How to Live," giving the weights ot clothing, may prove useful for reference:

AVERAGE WEIGHT OF CLOTHING (MALES)

COAT AND VEST

	Tropical	*Mid-Summer*	*Winter*
Small	1 lb. 3¾ oz.	2 lbs. 1½ oz.	2 lbs. 8¼ oz.
Medium	1 lb. 5 oz.	2 lbs. 4¼ oz.	2 lbs. 11¼ oz.
Large	1 lb. 8 oz.	2 lbs. 9 oz.	3 lbs. 1 oz.

ALL OTHER CLOTHING INCLUDING SHOES

	Tropical	*Mid-Summer*	*Winter*
Small	2 lbs. 8 oz.	2 lbs. 9 oz.	5 lbs. 9¼ oz.
Medium	3 lbs. 11 oz.	4 lbs. 11 oz.	5 lbs. 14 oz.
Large	4 lbs. 1 oz.	5 lbs. 1 oz.	6 lbs. 4 oz.

AVERAGE WEIGHT FOR ALL SEASONS

COAT AND VEST

Small	1 lb. 15 oz.
Medium	2 lbs. 1⅗ oz.
Large	2 lbs. 6 oz.

ALL OTHER CLOTHING INCLUDING SHOES

Small	4 lbs. 3⅕ oz.
Medium	4 lbs. 12 oz.
Large	5 lbs. 2 oz.

AVERAGE WEIGHT OF CLOTHING (WOMEN)

DRESS AND CORSETS

	Tropical	*Mid-Summer*	*Winter*
Small	1 lb. 11 oz.	2 lbs. 8 oz.	2 lbs. 11 oz.
Medium	1 lb. 14 oz.	2 lbs. 12 oz.	2 lbs. 15 oz.
Large	2 lbs. 4 oz.	2 lbs. 15 oz.	3 lbs. 5 oz.

ALL OTHER CLOTHING INCLUDING SHOES

	Tropical	*Mid-Summer*	*Winter*
Small	1 lb. 15 oz.	2 lbs. 10 oz.	3 lbs. 6 oz.
Medium	2 lbs. 1 oz.	2 lbs. 12 oz.	3 lbs. 8 oz.
Large	2 lbs. 12 oz.	3 lbs. 6 oz.	4 lbs. 6 oz.

AVERAGE WEIGHT FOR ALL SEASONS

DRESS AND CORSETS

Small	2 lbs. 4⅗ oz.
Medium	2 lbs. 8⅗ oz.
Large	2 lbs. 13⅕ oz.

ALL OTHER CLOTHING INCLUDING SHOES

Small	2 lbs. 10⅕ oz.
Medium	2 lbs. 12⅕ oz.
Large	3 lbs. 8 oz.

In regard to the Flack tests, the relationship of the findings to physical condition have not been so minutely worked out as in the case of vital capacity. It is notable in Flack's report that all the fit classes registered well above 100 in the expira-

tory force test, yet the minimum normal was fixed at 80. The fatigue tests average 48 among fit classes; low normal has been fixed at 40. The breath-holding tests average 66 seconds among the fit classes; low normal would here appear to be between 55 and 60. In our own observations we have found very low readings for women in the expiratory force tests; normal, fairly active women showing normal vital capacity had difficulty in registering 80 and low normal would appear to be in the neighborhood of 50. A woman who cannot register 50 in this test or a man who cannot register 100 are open to question as to their physical condition.

All of these tests are liable to error through lack of intelligent coöperation on the part of the subjects, and unless one can be sure that there is full coöperation the reading should be rejected and excluded from any statistical studies.

Practical difficulties will be encountered in the examination of women if chest measurements are employed in view of the wide variation in mammary tissue. We have met this difficulty in our own observations and Professor Dreyer's formula for correcting these readings does not work in our practice. Perhaps the chest volume, as suggested by Garvin, Lundsgaard, and Van Slyke, would be a more accurate factor for women but this likewise involves the taking of so many measurements that the probability of error on the part of the average observer is increased. The body surface in relation to height is perhaps the safest method. The standards are lower than the Dreyer tables by 7% for women but from our observations I believe these standards are more accurate.

The risk of applying these tests in advanced disease must be considered. J. A. Myers states that Professor Dreyer has observed no unfavorable reaction, but our observation shows to the contrary, and we advise caution in applying any of these tests in advanced pulmonary or circulatory conditions. We have made a considerable number of observations on the influence of these tests on blood pressure and find that the blood pressure is increased from 10 to 30 millimeters during the test. In such conditions the patient should be asked to do only what

can be done without manifest distress or unfavorable reaction, and in this way secondary standards for advanced cases might be established. Just as it is hazardous and unwise to ask a patient suspected of myocardial insufficiency to hop 100 times on one foot, so is it unwise to push these tests to the limits necessary for correct readings in average people.

As to apparatus, spirometers are not generally available on the market. An instrument such as is exhibited here to-day is made by the Sanborn Company at Boston but is quite expensive. There has been so little demand for these instruments that no convenient, economical and simple portable form has been made available. The instrument made by Boulitte in Paris and recommended by Dreyer is no doubt more convenient and portable than the standard type. If these tests are carefully tried out in various clinical centers, head offices and important branch offices of insurance companies and in other places where physical conditions are assessed, and their apparent value confirmed, no doubt they can be more commonly utilized by the average examiner. The present widespread use of the sphygmomanometer may be credited to life insurance medicine. By requiring the blood pressure in examining for life insurance, this simple but valuable clinical resource has finally passed into the hands of practically all physicians, although it is not so many years ago when it was considered chimerical to attempt to secure these readings from the average examiner. Bearing carefully in mind, as I have already suggested, the limitations of all efficiency tests, it would seem that we are leaving something very important undone if we do not ultimately develop some simple and practical method of testing respiratory efficiency as a supplementary procedure to auscultation and percussion just as we have developed circulatory tests, which do not by any means take the place of the physical examination of the heart but often give information which could be obtained in no other way.

I regret that the invitation to discuss this paper was not received in time for me to make more extensive original observations as to the value of these tests. I present the following

studies, however, for what they are worth and I can promise you that by next spring we will have from five to ten thousand observations on people of all classes, examined with great care by our Head Office physicians and technicians, from which it may be possible to draw more positive conclusions than can now be confidently drawn from the comparatively meager data available.

Fifteen substandard men, including some organic cases, gave the following readings:

Vital capacity for Class	13%	
Vital capacity below Class A	20%	
Vital capacity compared to surface area standard	24%	
Expiratory Force	67	mm. (33% below standard)

Ten men apparently free from important defects:

Vital capacity for Class	2%	
Vital capacity as compared to Class A	11.5%	
Vital capacity as compared to surface area standard	14 %	
Expiratory Force	70	mm. (30% below standard)

Twenty substandard women, including some organic cases:

Vital capacity for Class	21%	
Vital capacity as compared to Class A	30%	
Vital capacity compared to surface area standard	29%	
Expiratory Force	34	mm. (32% below standard)

Ten women apparently free from important defects:

Vital capacity for Class...........	10.4%	
Vital capacity as compared to Class A..........................	17.5%	
Vital capacity as compared to surface area standard............	16 %	
Expiratory Force.................	40	mm. (20% below standard)

As to the sufficiency of the material on which Professor Dreyer has based his elaborate tables, we have learned to respect the testimony derived from small groups carefully selected, and it may be that as Professor Dreyer claims future investigations will not materially alter the standards he has fixed. I believe it to be true, however, that as Dr. Dublin has suggested, the further use of these tests may show them to have even a wider range of usefulness than Professor Dreyer claims. On present evidence, however, I think we must regard them simply as tests, and not as offering infallible testimony with regard to underlying physical fitness or longevity.

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Dr. Symonds—Mr. President: In regard to the question of the assessment of physical fitness, I think we have all been very much interested in Dr. Dublin's presentation of Dr. Dreyer's work, and in the further results that were checked up by Dr. Fisk. Personally I haven't any use for the spirometer because that is a game that could be beaten by an educated applicant, and nowadays our applicants are getting to be thoroughly educated. The element of stem length, however, always appealed to me, because for years we have held the view that overweights with long stems were better than those with short stems, and chest measurements and weights are all factors. The whole subject is one that is new, it is one that does not permit of any mortality work because I don't think any of us have any records of stem lengths. Does anybody here take the height sitting down?

Dr. Muhlberg—We kept record of a considerable number of cases where the height was taken sitting down.

Dr. Symonds—That would perhaps give us a basis, but I think the matter should be referred to a committee to study and to investigate and report, either to us or to the Executive Council, and see what can be made out of it. I make a motion therefore to that effect, and I would add the wish that if the motion were carried, Dr. Dublin be incorporated as an associate with the Committee.

Dr. Daley seconded the motion and it was carried.

Dr. Knight—It gives me a great deal of pleasure to introduce to you Dr. Harvey R. Gaylord, Director of the Gratwick Laboratory Research Hospital up in Buffalo, N. Y. I happened to be up in the northern part of the State the early part of this year and, in talking with Senator Sage, I discovered the regard in which Dr. Gaylord is held by the best men in the State from the governor down. I knew that Dr. Gaylord had been in Germany this past summer, studying the treatment there by radiation, and I thought it would be fine if he would come down here, and talk to us on the subject of cancers.

CANCER

By Dr. Harvey R. Gaylord

Dr. Knight has told you of the pleasant association which we enjoyed some years ago with the Metropolitan Life Insurance Company, and that they furnished us with funds with which we started certain valuable mouse-breeding experiments. That work has been continued and is today giving promise of new and interesting developments in the field of cancer research.

It is a difficult matter to deal in a general way with the cancer problem. Its ramifications have led us into so many strange fields that one, at best, can only touch lightly upon the high points or deal more in detail with some particular phase of the subject.

In addressing an audience of this kind, men who deal with the problems of sickness in a statistical and broad way, I realize how indiscreet many statements with regard to cancer are. I propose to submit to you in detail the results of a trip of investigation which I made during the past summer for the

purpose of appraising the clinical results of radio therapy in Germany. This trip disclosed several years of extensive work upon a complicated and vital phase of the cancer question and I feel that you will agree with me, after I have presented the facts, that interesting advances have been made in this direction in Europe.

One must guard against optimism and over-enthusiastic statements in this field, and yet it is possible to be sensational by denying the advances made and understating the facts. Within the last few days I have seen a well-known cancer authority quoted in the daily press as having said that not one case of cancer in a thousand has been cured by radiation. When we realize that basal cell epithelioma of the skin, which was difficult to deal with successfully by surgery, is now regularly and permanently cured by the X-ray and, with the more recent technique, with one dose, it must be clear that the gentleman, in making such a statement must have excluded the results of radiation in this type; and when I shall later give you the statistics of the results of radiation with radium and X-ray in cancer of the uterus from the Gynecological Clinic at Freiburg, I am sure that it is not difficult to calculate that epithelioma of the skin and cancer of the uterus, viewed in the light of successes in these two fields, certainly constitute more than one-tenth of one per cent. of cancer.

In the days when I started in cancer research the disease was considered as a fatal malady, which, when not alleviated by early surgical operation was essentially hopeless. Today we can certainly deal successfully with many cases which are beyond the help of surgery and what seems to me should be of great interest to medical directors of life insurance companies, is that it is now amply demonstrated that radiation either with radium or X-ray or combined gives us the greatest palliative we have ever had for cancer; that the lives of cancer cases can often be increased from one to four years with the modern technique in a high percentage of the cases treated.

It is this particular phase of modern cancer therapy which I desire to impress upon you. Palliation may not seem

to be a great gain but I shall tell you of at least two cases where it has meant everything to the individual and the individual's family. There are certainly many such cases in the records.

Before dealing more specifically with therapy we might point out some of the directions in which our knowledge of cancer has greatly improved.

As you know we have known for years of a type of cancer of the bladder occurring in the field workers in Egypt. This disease is known as Bilharzia Disease and is caused by the embryos of the trematode or fluke worm Schistosma Hematobium which gain entrance through the skin between the toes of the feet, penetrate into the circulation where the females lay their eggs which are carried by the blood current to various organs, being deposited in quantities in the walls of the bladder and the lower end of the large intestine. In the bladder they set up chronic irritation with the development of granulation tissue, papilloma, sarcoma and carcinoma.

Some years ago Professor Febiger of Copenhagen discovered and worked out the etiology of epithelioma of the esophagus and upper portion of the stomach in rats caused by a minute nematode worm which lives part of its life in cockroaches. When the cockroaches are eaten by the rats the embryos of these worms penetrate to the esophagus and wall of the stomach and there set up epithelial proliferation which he has shown to be genuine neoplasms as the cells penetrate into the circulation and produce metastases. He has succeeded in transplanting this neoplasm and it has been found to fulfill the criteria by which we distinguish a malignant growth.

Minute nematodes have been found occasionally, within or in the neighborhood of mouse carcinoma and Borrel, who first called attention to them, suggested that some day the distribution of a nematode might be found to determine the prevalence of a certain type of cancer in that neighborhood. This forecast, years later, Febiger found that he had substantiated for the reason that the hosts of the minute nematode causing the carcinoma in the rats was the West Indies cockroach and not

the ordinary continental variety. These cockroaches were only found in Copenhagen around a sugar refinery which received its supplies of raw sugar from the Danish West Indies and no where else in Copenhagen was he able to find rats which were infested with these worms and had developed carcinoma therefrom.

Nematodes and other worms have come to be objects of interest in the study of neoplasms. In the so-called carcinoma of the thyroid in the salmonoid fishes which I studied some years ago with Marsh we found that microscopic nematodes played a suggestive rôle and more recently Kopsch of Berlin studied a nematode which infects frogs, in which he has found epithelial proliferation and one or two cases of cancer.

This is one direction in which cancer research has led us. There is a group of neoplasms, sarcomata in chickens which Dr. Peyton Rous of the Rockefeller Institute discovered some years ago and which have been intensively studied in many laboratories. In this group we have a genuine neoplasm caused by a filterable virus. That the disease is widespread is shown by its having been observed and studied in Japan within the last few years.

Teuschlander of Heidelberg has found a spontaneous case in chickens in Germany, and has been able to identify this disease as that described by Rous by repetition of many of his experiments and observations thereon.

We have never been able to obtain a filterable virus from a mammillary carcinoma or sarcoma.

Some years ago Japanese succeeded in producing epithelioma of the skin in rabbits and mice by painting the surface with tar and recently, also, in London at the Imperial Cancer Research Fund a great series of mice were being subjected to this experiment by simply painting tar upon the shaved surface of the skin of the backs of the mice at bi-weekly intervals over a period of some months. A large cutaneous horn will develop, at the base of which in 30% of the treated mice, true epithelioma develops. These growths frequently metastasize and

may be transplanted. There is no doubt that they are malignant neoplasms.

Having put before you these facts, examples so widely spread from each other in etiology it must be clear that that great group which we have been in the habit of treating as an entity and calling "cancer" is not one process but many distinct diseases. It is the realization of this fact, obtained by years of painstaking study of various types of neoplasms in the lower animals which has finally brought us to a realization of the stupendous nature of the problem and which explains how progress has been so slow and why statistics which do not recognize these fundamental considerations have been so confusing. We shall make progress from now on in proportion as we understand that we cannot mix up clinical data and therapeutic results in general statistics under the caption of cancer. Some types of cancer respond readily to the newer means of treatment such as radiation while in others practically no success has thus far been obtained. The successes in some types of cancer should not lead us to the general conclusion that all cancer may be successfully treated by the means at hand and failure with other types should not lead us to the pessimistic conclusion that these means are a failure as some surgeons at present seem inclined.

In dealing with the question of therapy we must realize that both surgery and radiation are local means of attacking this problem. It is an accepted truth that cancer is, in the beginning, a local disease. In the type of neoplasm of which we know the most, the chicken sarcoma, where we have been able to deal with the agent causing the neoplasm, this has been shown to be true, and when the disease can be attacked locally, when it is still a local lesion, it can be cured, but it is impossible for any clinician to tell whether even an early cancer is still local, and the disheartening failures of surgery and the disappointments of radiation have again emphasized that there is a background to the whole question of cancer therapy which I think may properly be spoken of as the "immune question." Why do certain individuals develop cancer and why do a certain

proportion of cancers respond brilliantly to therapy while certain other cases, apparently no further advanced, do not respond at all. This is, perhaps, the great question for cancer research.

In 1904 we had the good fortune in Buffalo to discover that mice inoculated with mouse cancer, could, in a certain per cent. recover spontaneously and when so recovered remain immune to further inoculations and, growing from this and other early observations, a great many facts have been accumulated which indicate that the resistance of the individual to the disease is a highly important factor to the success of both surgery and radiation. In a general sense from what I have seen of a large mass of material both in this country and abroad, I am more impressed than ever with the fact that our expectations must not be placed too high for any local means of treating this group of diseases.

Palliation is the most certain result to be obtained by radiation but a certain proportion of the treated cases make a complete recovery with a restoration to health. What percentage are ultimately cured it is impossible to determine at present for the reason that our means of applying radiation are steadily improving and our technique is constantly changing.

You know that it is possible to breed mice by selection so that as high as 90 or more per cent. of the females will develop cancer of the breast when they reach the cancer age. These mice are kept in a highly artificial environment and generously fed and live under circumstances which might be compared to those of civilization for human beings.

As you know for many years past the mortality statistics for cancer have been steadily mounting the rate of increase during the last few years has diminished and it may interest you to know that the statistics now available from Germany indicate a falling off in the mortality from cancer during the years of the war. By years it is as follows:

1914—52,205 deaths from carcinoma
1915—50,361

1916—51,527 deaths from carcinoma
1917—51,058
1918—51,842

In men 23,494 died in 1914 and in 1918, 21,804.

Men actually drawn into the military service showed a steady rise from year to year but, of course, this is not significant as it is highly probable that men of increased age were more frequently taken in the later years of the war for military service.

However for the statistics available the rapidly rising rate preceding the war in Germany shows a falling off in the percentage increase during the war period.

In our mouse-breeding experiments we have for some time considered and have been studying the question of food intake to work performed, realizing that as cancer was not a disease of the poorly nourished it was possible that the susceptibility to the disease might well be influenced by conditions approximating those of civilization in man.

We have lately been studying the relation of nematodes and other worms to the incidence of cancer in mice and, our biologist, Mr. M. C. Marsh will, I hope, have interesting data to publish in the near future.

I shall now pass to the more detailed portion of my report to you and tell you of what I found in my recent trip to Germany with regard to so-called deep therapy. It is a rather startling thing to be able to see the product of six years of progress in a country as well equipped for scientific research as Germany has been.

Report on Status of Deep Therapy in Germany

Summer of 1921

CLINICS VISITED

Munich

The Gynecological Clinic of Professor Döderlein.
(Physicist—Dr. Friedrich Voltz)
Surgical Clinic of Professor Sauerbruch.

Freiburg ii Br.

The Gynecological Clinic of Professor Opitz.
Surgical Clinic of Professor Lexer.
Medical Clinic of Professor de le Camp.
Physical Institute of Professor Friedrich.

Frankfurt

University of Frankfurt.
Surgical Clinic of Professor Schmieden.
The Gynecological Clinic of Professor Seitz.
Physical Institute of Professor Dessauer.

Berlin

The Gynecological Clinic of Professor Bumm. (Dr. Warnekross)
The Cancer Research Institute at the Charité Hospital.
The Private Clinic of Professor Halberstadter.

APPARATUS

As a careful investigation of the status of German X-ray apparatus for deep therapy was made last year by Dr. Coolidge of the General Electric Company, it is not necessary to go into the details of the German construction other than to say that since Dr. Coolidge's inspection last year the Veifa Company have increased the capacity of their apparatus so that it is now capable of maintaining 220,000 volts and that new water-cooled Coolidge tubes are being manufactured by the firm of Veifa. Such an apparatus has been recently installed in the Surgical Clinic of Professor Lexer at Freiburg and a similar apparatus has been ordered for the Surgical Clinic at Frankfurt.

The clinics that I visited are equipped with from two to six outfits. The clinic at Erlangen which I did not visit has eight outfits in operation.

The apparatus can be divided into three types:

1. That using the Coolidge tube such as the Veifa Intensive Reform apparatus (Frankfurt), Siemens and Halske (Berlin),

The Sanitas Apparatus (new and experimental apparatus, so-called hard ray apparatus).

2. The Symitrie Apparatus (Reiniger, Gebbert and Schall—Erlangen) which uses the self-hardening gas tube.

3. The Silex apparatus of Koch and Sterzel using the Lillienfeld tube.

Of these three types of apparatus it may be said that the gas tube arrangement employed with the Symitrie apparatus has probably reached its highest development and is exceeded in capacity by the various types of apparatus using the Coolidge tube. The reason why so many installations of this apparatus have been made in the past has been due, I think, to the fact that it was the first in the field and that the manufacturer was able to supply tubes. Capacity of the apparatus is from 180,000 to 200,000 volts.

The apparatus of Koch and Sterzel is designed to be used with Lillienfeld tube. The Lillienfeld tube is said to be short lived, only 50 or 60 hours. This is a serious drawback. The advantage of the tube is that it will take 8 to 10 milliamperes and thus the exposure is shortened.

Types of apparatus using Coolidge tubes: Of these the oldest is the Veifa Reform Apparatus. This type of apparatus is rapidly replacing the gas tube Symitrie Apparatus and most of the institutes ordering new apparatus were installing this type.

The apparatus has been tested in actual practice and the first installation made in 1915 in Berlin was still in operation when I was there, rendering good service.

The other types of apparatus mentioned above under this heading are all new and experimental.

A Symitrie installation with all necessary accessories for operation costs today about $4000. A similar apparatus recently offered in the United States is listed at $8200.

During the past year rapid strides have been made in the United States in the perfection of interrupterless apparatus. Under the direction of Dr. Coolidge the Victor Company of Chicago have placed on the market an outfit consisting of a transformer hooked up with a Snook cross-arm type rectifying

four-point switch. The apparatus itself is capable of producing 280,000 volts which under American methods of measurement is equal to a 20-inch spark gap. Upon a recent investigation of this type of apparatus in operation at Schnectady it appears that the new tube perfected by Dr. Coolidge can readily take 200,000 volts and stand a load of 8 milliamperes.

There is reason to believe that this type of rectification is easier on the tube than the single rectifying switch employed with the Veifa Apparatus. In view of the desirable characteristics of the recently perfected American apparatus combined with the marked improvement in the capacity of the tubes recently produced by Dr. Coolidge I should consider that in many particulars it is superior to the best German apparatus.

All of the types of apparatus thus far described are actuated by alternating current using some type of rectification. Dr. Hull of the General Electric Company and Professor Duane of Harvard have been experimenting for some years with types of direct current apparatus. Professor Duane's apparatus uses 500 cycle a. c. current which is stepped up by specially constructed transformers. Dr. Hull's apparatus uses 2000 cycle a. c. current by means of four kenotrons and condensers. In both of these types of apparatus the current is converted into d. c. current having the advantage of definitely fixed voltage. The apparatus at present constructed will be capable of handling d. c. current at 200,000 volts. This is practically equivalent to 225,000 volts on the interrupterless a. c. apparatus. The d. c. apparatus is very expensive in first outlay and the advantages are problematical. Whether the apparatus will become a commercial apparatus suitable for installation generally remains to be seen.

There was, at first, some question as to whether the new tubes would take the direct current as easily as the alternating.

Tests up to the present time seem to indicate that there is no difficulty in this particular so it is possible that a new type of American apparatus may be perfected using direct current. At the present time I should consider that any hospital pro-

posing to install deep therapy would act wisely in purchasing American apparatus of the a. c. variety.

TUBES

The demand placed upon the large Coolidge tubes manufactured in Germany has been such that in the past puncturing has been a serious trouble. With the perfecting during the past year of Fürstenau water-cooled Coolidge tubes this difficulty seems to be greatly diminished. In the United States we shall be unable to obtain commercially the German tubes as they are only manufactured in Germany under a patented license for use in Germany alone. Any installation in the United States will have to secure its tubes from the General Electric Company.

DEEP THERAPY
(so-called)

There has been considerable discussion in the United States on the part of X-ray operators as to whether or not the Germans have developed a type of X-ray therapy which is really different from and capable of producing results distinct from those obtained in America. This question, I believe, can now be answered in the affirmative. On the whole the German apparatus uses higher tension current than is used in the United States, and the apparatus of the last year produced in Germany without question produces effective voltage over 200,000. Such a voltage is beyond the capacity of the typical American apparatus and will only be reached by the newer apparatus being constructed in the United States now.

The development in the clinical application of hard X-rays, by which is understood the so-called science of deep therapy, has resulted from elaborate and painstaking investigations on the part of German physicists who have carried out a long series of carefully planned experiments and innumerable measurements. It is in this particular that the Germans have

outstripped us in the past six years. There is still much to be done and learned about the physical side of deep therapy. The problem is by no means exhausted, but the Germans have developed a science by which the operator can estimate with considerable certainty the amount of radioactivity which can be obtained at various depths in the tissue. The apparatus employed in Germany has been constructed with the view to securing greater voltage, thus producing harder rays from the X-ray tube than were before obtainable.

CLINICAL RESULTS

This phase of the question is the most difficult of all. Since the beginning of what may be called X-ray therapy in 1902, by the first publication of Nicholas Senn in which leukemic tumors were made to disappear after exposure to the action of the X-ray tubes of those days, the course of X-ray therapy has been punctuated by occasional astonishing successes where tumors have disappeared when treated with amounts of X-ray much smaller than those employed today, and with rays from tubes incapable of giving out the hard rays of the present day tubes. The question is, therefore, not to be determined by a collection of spectacular results of rare occurrence but can only be solved by determining to what extent uniformity of result is obtained and whether deep seated lesions previously beyond the reach of the X-ray are now successfully treated.

After carefully studying the field I am convinced that the technique of the Germans with the present apparatus actually constitutes a marked step in advance in this latter particular.

CANCER OF THE UTERUS

The most striking example of uniform success with deep therapy is in cancer of the uterus, where, however, we find that most of the German clinics use radium in combination with X-ray. However, I have seen in several clinics cases of cancer of the uterus (carcinoma of the cervix) of advanced type which

have been successfully treated with X-ray alone from without; one case cured six years and four cases cured more than three years.

Cancer of the cervix and cancer of the body of the uterus are perhaps the most favorable of all types of cancer for treatment with radiation. The uterus lies centrally and can be reached by radiation from before and behind and from the sides. To this may be added that with radium it can be successfully radiated from within outward. The difficulty with radium treatment alone in cancer of the uterus is in reaching from within involvements of the broad ligaments and the sacral group of lymph nodes posteriorly. By employing large fields from in front and behind the entire pelvis can be given a more or less uniform dose capable of reaching successfully these outlying involvements. The following figures indicate the degree of success in combined radium and X-ray treatment in the Gynecological Clinic at Freiburg:

Statistics of the Freiburg Gynecological Clinic of Professor Opitz for the period January 1, 1919–January 1, 1921.

Total Number of Cases of Cervix and Portio-carcinoma treated: 60

of which 30 were clinically cured
13 were uninfluenced
17 died

BY GROUPS

Group I—Early and Operable—4 Cases
All clinically cured

Group II—Late Operable and Borderline—13 Cases
8 clinically cured
3 uninfluenced
2 died

Group III—Inoperable—39 Cases
18 clinically cured
9 uninfluenced
12 died

Group IV—Late Cases with Cachexia, etc.—4 Cases
1 uninfluenced
3 died

Total of Corpus-Carcinoma: 30
of which 23 were clinically cured
3 were uninfluenced
4 died

BY GROUPS

Group I—10 Cases
All clinically cured

Group II—9 Cases
All clinically cured

Group III—8 Cases
4 clinically cured
2 uninfluenced
2 died

Group IV—3 Cases
1 uninfluenced
2 died

In this clinic the technique has not been changed for two years; it being the purpose of Professor Opitz to determine definitely what may be expected over a period of years from the methods now employed. The technique at this clinic is based upon investigations of Professor Friedrich. Friedrich was originally associated with Professor Krönig who died during the war. Krönig was the first German gynecologist to treat cancer of the uterus with radium and mesothorium. Friedrich has recently been made full Professor in the University of Freiburg, has an independent laboratory with several assistants and has done an immense amount of accurate work. He has developed the use of a small ionization chamber attached through a cable to a galvanometer. By this means the quantity of radioactivity can be measured at a given point. In treating cancer of the uterus the ionization chamber is placed in the vagina as near the uterus as possible and the

operator reads off from the galvanometer the length of time which it takes to ionize the air in the ionization chamber. When one reading is taken the instrument is again set and when the final total amount of radioactivity desired has been applied the treatment is complete. This method obviates the necessity for determining the peculiarities of individual X-ray tubes. It is a quantitative method and one applicable to any type of apparatus.

It has been pointed out that the physical problems of radiation are not entirely solved. Until they are more or less definitely solved it will be difficult to speak with certainty about the biological problem, that of the action of the X-rays upon the tissue itself. Professor Friedrich contends that although we do not know exactly what the X-ray does to the cancer cells, this method of measuring radioactivity at depth comes as near indicating what is desired as any method yet devised. From the results seen at the Gynecological Clinic at Freiburg I am inclined to share this view.

It should be pointed out that at the Gynecological Clinics other types of malignant disease are treated than cancer of the uterus. Nearly all of them treat cancer of the breast, sarcoma and other types of malignancy; in fact as the gynecologists were the pioneers in the use of radiation most of the Gynecological clinics are, in reality, centers of deep therapy for the treatment of cancer in general.

It should be pointed out that at Freiburg, Munich and Erlangen no surgical operations are performed for cancer of the uterus and this has been the case for several years. In Berlin operations are occasionally performed but the majority of cases are treated by radiation.

At the Gynecological Clinic of Professor Bumm at Berlin I saw four cases of carcinoma of the uterus which had been cured from two to five years with the use of deep X-ray therapy alone. The case cured for five years was one of great interest, being a working woman of about 52 or 53 years of age. She had been treated by multiple fields through the abdomen. Dr. Warnekross who demonstrated the case showed me the records. The

patient had a portio-carcinoma which had involved the right broad ligament and adnexa. The uterus was fixed and enlarged, the case inoperable. After deep radiation, carried to the point where the skin over the several abdominal fields showed marked erythema, regression of the growth set in, which, in the course of a few weeks practically disappeared. The patient has gained 30 or 40 pounds in weight, was able to return to work in a few months and has remained robust and well ever since.

I examined a number of cases of this sort and have brought back the records of several cases where involvements of the broad ligaments on one or both sides have been followed by complete clearing up of the growth with restoration to health and freedom from recurrence over varying periods up to and including three years. A special case, the history of which I saw in Munich, was that of a patient with inoperable carcinoma of the cervix involving both broad ligaments which received one course of deep therapy, returned to her home, showed no signs of improvement at first, in fact for three months grew steadily worse, and then regression set in and ultimately she made a complete recovery and is today three years well and in good health.

I did not visit the clinic at Erlangen for the reason that Professor Wintz is extremely enthusiastic about deep X-ray therapy and I felt it wise to come away from Germany without having my point of view modified by too much enthusiasm. Incidently I had seen the type of apparatus employed at Erlangen in use at other clinics. The criticism of propaganda by manufacturers of X-ray apparatus originated largely from Erlangen where the firm of Reiniger, Gebbert and Schall supplied the equipment for the Gynecological Clinic of Professor Wintz.

At Munich I had ample opportunity to discuss the matter of the technique at Erlangen with Dr. Friedrich Voltz, until recently the physicist who was associated with Dr. Wintz in the scientific portion of his work at Erlangen. At the Gynecological Clinic in Munich both types of apparatus are used. The technique consists in standardization of the tubes by the

physicist. Curves for every tube are prepared showing the measured output of the tube and the character of the rays obtained. These charts are used by the operator in determining the voltage milliamperage to be employed.

In all of these clinics the application of deep therapy to the patient is controlled by trained medical men; that is, the patient is prepared, the relation and distance of the tube, direction of the focus, are all made by the physician. The watching of the patient, control of the apparatus during the time of exposure is made by female assistants, usually Sisters of Charity, as most of the German hospitals especially in Southern Germany are under the control of Catholic Orders.

The time of exposure ranges from 4 to 8 hours, depending upon whether the patient is exposed with one or two tubes at a time. With the introduction of the new water-cooled Coolidge tubes which at 200,000 volts will take 8 milliamperes, the time of exposure can be cut in two and with the general tendency to reduce the dosage one finds in certain clinics, especially the Medical Clinic at Freiburg and the Surgical Clinic at Munich and the Cancer Research Institute in Berlin, a tendency to decrease the length of time of exposure.

CANCER OF THE BREAST

Next to cancer of the uterus the most important field is cancer of the breast. The treatment for cancer of the breast is, in general, in Germany, operation preceded and followed by prophylactic treatment. There have been a good many cases of cancer of the breast which for various reasons were not operable and these have been treated by radiation. In many of these cases excellent results have been obtained by radiation alone, and I found the opinion in general that cases which had not been interfered with by surgical operation were more amenable to treatment than cases which had been operated and recurred.

There has grown up a controversy in Germany, resulting from a publication of Perthes of Tübingen, in 1920, purporting

to show that the results of operation alone, in the period in the Tübingen Clinic before prophylaxis was used, showed better results from surgery alone than in the period in which the cases were rayed for prophylactic purposes. It has been shown by critics of Perthes' publication that the technique used in the Tübingen Clinic on the majority of these cases cannot properly be classed as deep therapy, and that the whole technique was not as complete and thorough as that practiced in other clinics. Anschutz (Kiel), whose technique for a number of years has been of a modern character, has published statistics in direct controversion of those of Perthes, showing that the evidence is in favor of radiation. This agrees with the experience of the other clinics in Germany.

Prophylaxis as practiced in Germany consists in giving a full carcinoma dose in exactly the same manner as if a carcinoma were present. Before and after the operation this full radiation is given and repeated at the end of one-quarter, one-half and three-quarters of the year afterwards. At the Frankfurt Clinic under Dr. Holfelder in the last two years between 80 and 100 cases have been radiated with only two recurrences.

Perthes claimed that only 26% of permanent cures had resulted in surgical cases combined with prophylaxis. His opponents are of the opinion that his method of radiation is too weak and he has, in fact, been applying what are known as stimulating doses. In opposition to Perthes, Techy (Marburg), Payer (Leipzig), Anschutz (Kiel), Mueller (Rostock), and Wendel (Magdeburg) found that the statistics of permanent cures were between 45 and 67% and all of them agreed that the difference was in the technique.

The experience in Frankfurt is that recurrences of the thoracic wall are regularly disposed of by deep therapy. A certain proportion of recurrences of the pleura are disposed of. If the radiation is carried out with too soft rays it is very likely to produce metastases of the skin and in the muscles. These can, in a considerable percentage of cases, be disposed of by proper deep therapy. I have found that in all the clinics it is considered much more difficult to influence cases that have

been previously inadequately rayed than to produce favorable results with cases which have not been rayed.

Lengthening the life of cases of carcinoma of the breast by radiation seems to be an unquestionable fact and agrees with the experience in the United States. The consensus of opinion was that the life period can be lengthened from three to four years by proper radiation.

At the Cancer Research Institute in Berlin I found a large number of cancers of the breast were being rayed and with exceptionally good results where the radiation is combined with chemotherapy. The patient receives before radiation an intravenous injection of an iodine preparation known as "Alival" and is treated constitutionally during the period of treatment with Atoxyl, an arsenic preparation. I was told at the Cancer Institute that approximately 80% of the breast cases were now reacting favorably to radiation.

CANCER OF THE INTESTINE

These cases are all operated, either removed or an anastomosis around the tumor is made when possible. The experience is that cancer of the cecum and sigmoid respond more favorably to radiation than cancer of the rectum. Where possible the cases are radiated with full dosage and eight weeks later are operated; in between, an artificial anus is made where necessary. After operation the full dose is again given as if the growth were present.

At the Frankfurt Clinic cancer of the rectum is treated in the following way: the tumor is radiated, an artificial anus in the median line is made and at the time of operation an inspection of the abdominal cavity especially to determine the presence or absence of metastases in the liver. The lower end of the gut is invaginated and sunk. Eight weeks later a modified Kraske operation is made, laying open the course of the rectum from behind and amputation of the coccyx to enlarge the field and an anatomical dissection of the rectum and the tumor carried up to the peritoneum which is opened. The free in-

vaginated end of the gut is drawn down, the peritoneum is closed and the whole is dissected out, including the anus. After the operation, when healing is complete, a further radiation of the area with a full dose is made. The success of this method of procedure has brought to light very interesting possibilities with regard to metastases of the liver. I personally inspected two cases at Frankfurt where metastases of the liver were present at the time of the abdominal operation, these metastases having been afterward radiated successfully. The histories of the two cases are as follows:

Case 1. Inoperable carcinoma of the rectum, size of a goose egg, firmly fixed to the sacrum. Three large metastases, size of English walnuts at inspection at the time of operation by Professor Schmeiden, one metastasis on the border of the right lobe, one on the border of the left lobe. The primary tumor in the rectum was not operated but radiated. Eight days later the entire liver was radiated. This was followed by great abdominal distress and suffering by the patient with general weakness. Following, the symptoms of radiation rapidly cleared up and the patient who had previously been confined to bed was so improved in three weeks that he was able to return to work after many months of sickness. Eight weeks later a second radiation of the primary growth was given and it is at the time of inspection, July 5th, the size of a hen's egg, no longer ulcerated but the finger can be passed behind it. It is still adherent to the sacrum and not yet operable but distinctly reduced in size. The patient has gained 12 pounds and is in generally improved condition. The metastases which were previously palpable through the abdominal wall are no longer demonstrable; it is obvious that they are greatly reduced if not disposed of, as in the period that has elapsed since inspection they should have increased in size. Patient received on July 5th a further radiation of the rectum. Three months from date it was proposed to again radiate the liver.

Case 2. Inoperable cancer of the rectum directly above the sphincter. In performing a colostomy in the median line a number of metastases on the margin of the left lobe of the liver

and ligamentum hepatus were observed by Professor Schmeiden between a hickory-nut and walnut in size. Patient was radiated, both the primary tumor and the liver, in September, 1920, and a considerable amount of mucous discharge followed the radiation of the rectum. Examination of this patient on the 15th of October showed that the primary tumor had gradually decreased in size. Following the radiation of the liver there was no reaction other than vomiting which quickly ceased. The primary tumor ceased to grow and in May, 1921, the general condition of the patient had so markedly improved, having gained 20 pounds in weight, that the tumor being no longer adherent was removed by operation. The patient appeared to me to be in excellent health. He is a locomotive engineer and has for some time been following his vocation.

CANCER OF THE STOMACH

I was unable to find at the various clinics of Germany that uniform or satisfactory results have been obtained by deep therapy for cancer of the stomach. It is recognized that it is very difficult to radiate the stomach without injury to the adrenals. During the past six months at the Schmeiden Clinic four cases of cancer of the smaller curvature of the stomach have been radiated with remarkably good clinical results. By fluoroscopic examination it has been determined that the tumors have not increased in size, symptoms of obstruction have disappeared, the gastrostomy openings have closed and the patients have returned to work. Widely diffused carcinomas of the greater curvàture are not suitable for radiation. The more scirrhous types of cancer when confined to the lesser curvature are the ones which respond favorably.

CANCER OF THE ESOPHAGUS

The most uniform results in successful radiation of cancer of the esophagus I found at the Frankfurt Clinic. Here the technique has been refined during the last year as it was held

that radiation with radium gave very unsatisfactory results. During the past year 10 cases have been radiated, all of which have responded favorably, the tumors have decreased in size and subjective symptoms greatly improved. The localization of the tumor is determined by the fluoroscopic screen, patient being given a bismuth mixture to swallow, the localization being marked upon the surface of the body. Radiation is carried out by small fields, 6 by 10 cm. or 6 by 12 cm. according to the extent of the carcinoma. Eight or nine fields attempting to go completely around the growth are given and great care is given to protect the heart.

CANCER OF THE TONGUE

Here results with deep therapy do not seem to be as successful as the treatment with radium. The difficulty of securing homogeneous radiation at depth is greatly complicated by the air cavities of the mouth and pharynx. At most of the surgical clinics cancer of the tongue is operated. At Frankfurt marked improvement in the treatment of cancer of the tongue, tonsil, cavities of the mouth and larynx during the last year has been made by re-enforcing the region to be radiated with paraffin. In this group a collar of paraffin is placed about the neck, building the neck out to the margin of the jaw, thus securing a mass of sufficient area for the application of small fields, usually 9 by 12 cm. Previously regressions were not successfully produced; by this method the results are greatly improved. Three fields are given on each side and one from the back of the neck. From the cases I saw and from the histories I reviewed I believe that the American technique with implantation of emanation is superior to radiation alone; however, radiation of carcinomatous glands of the neck with radium packs could be replaced by deep therapy.

TUMORS OF THE BRAIN

Astonishing results have been accomplished by the radiation of brain tumors. Radiation of the brain is sometimes followed

by edema with compression symptoms. At Frankfurt a decompressing operation is performed before radiation. After the localization of the tumor by the neurologist, and when it is concluded that it is impossible to attack the tumor by operation, the localized field is rayed. It has been found that when the tumor was a gliosarcoma with round cells that it responded remarkably to radiation, decreasing in size and the symptoms produced by the pressure of the tumor rapidly disappearing. The results, as a rule, are temporary, but are followed in some cases by as much as a year of respite. Simple gliomata especially when extensive do not react well to radiation. I saw the history of a case at Frankfurt of an English business man who was brought to the clinic nearly blind and mentally incapacitated. A decompressing operation was done and he was rayed, as the case was inoperable. Following radiation his sight returned, his mind became clear and vigorous and he returned home where he was able to close up his business and made a will which he had not made, and for a year he had no return of symptoms. Within the last few months symptoms have begun to recur and at the time of my visit to Frankfurt he had returned for further radiation. The value of radiation of a brain tumor in this case is quite clear.

CANCER OF THE BLADDER

Carcinoma of the bladder responds especially well to deep radiation with the exception of aniline carcinoma of the bladder. In Frankfurt a number of cases of the latter had been treated; they did not respond. Electrocauterization is extensively used at the Frankfurt Clinic in treatment of cancer of the bladder. On the whole I believe that the American technique with implantation of emanation is superior to radiation from without. A case of diffuse carcinoma involving the entire bladder wall was reported to me at Frankfurt where four weeks following radiation remnants of the tumor were passed through the urine in large quantities. At the end of three months a cystoscopic examination showed no evidence

of growth in the bladder and the patient is now over a year free from bladder symptoms.

CANCER OF THE PROSTATE

Carcinoma of the prostate responds markedly well to deep radiation. Frequently the case, after the reduction of the size of the tumor, is operated. Where the deep therapy is not overdone there appear to be no marked adhesions to complicate the operation.

Hypertrophy of the prostate has been radiated without markedly favorable results. Hypertrophy of the third lobe of the prostate in the bladder has been radiated and greatly decreased in size, sometimes practically disappearing, but usually the results are complicated by dense fibrous tissue remaining which frequently complicates the condition at the opening of the urethra in the bladder. The whole procedure is not considered very successful.

BRONCHIAL CARCINOMA

Bronchial carcinoma appears to be one of the most favorable cancers with regard to deep therapy. In several clinics I learned that almost without exception bronchial carcinoma disappears completely under deep therapy. The only complication appears in cases that have been insufficiently rayed in the beginning.

SARCOMA

In several of the centers of radio therapy in Germany sarcoma is no longer operated. In Frankfurt, where Professor Schmeiden still continues to operate all cases of operable cancer, *no operations whatever are performed upon cases of sarcoma.* It is well known that sarcoma responds to smaller dosage than does carcinoma, and the experience with the disease is such that the chances of permanent cure are much better with radiation than with surgery.

TECHNIQUE AND DOSAGE

In general it may be stated that the method followed for dosage is to attempt to give a full so-called carcinoma dose and that it is realized that this dose must be uniform throughout the growth to be treated. It is believed in Germany that if voltage is too low and the resulting rays from the tube are not sufficiently penetrating, radiation of deeply seated tumors with rays of insufficient penetration results disastrously. This is due to the fact that the absorption of the rays decreases with depth. Thus the growth receives in its most superficial aspect more radiation than do the deeper parts. Unequal dosage of this sort results in stimulation of the part of the growth which receives insufficient dosage.

As we have noted at Freiburg the radiation is quantitatively measured with the ionization chamber. Aside from cancer of the uterus this is difficult to accomplish as the measuring instrument can only be placed beneath the patient when radiating from above and does not actually measure the amount of radiation in the growth. This has to be calculated from the amount measured on the surface, away from the tumor. Ordinarily the standardization of a tube is accomplished by using a so-called water phantom, it having been determined by physicists that water offers practically the same resistance to X-rays as the human body.

In all clinics in Germany as in the United States the basis of biological measurement of radiation is the erythema dose. There is some difference in the different clinics as to what constitutes the erythema dose just as there is in this country. In general it is supposed to be that amount of radiation which will produce upon the abdominal skin of a patient of middle age a certain amount of redness, in fact a light burn of the first degree.

In standardizing a tube the amount of radiation, at a given voltage, with a given filter, at a given distance from the skin, which will accomplish an erythema dose, is determined. Then, by means of the water phantom which is simply a vessel of

given dimensions, the amount of radiation which penetrated to given depths is measured. The amount of radioactivity which will penetrate to a given depth is determined by certain factors: First, the distance of the tube from the patient. This is due to the geometrical enlargement of the field, the output of the tube being distributed over a larger surface the further it is from the surface. The rule is the amount of radiation decreases as the square of the distance. The amount at a given distance is diminished by the absorption of the tissue through which it passes. These two losses are somewhat reduced by the effect of scatter rays. The human body is opaque to X-rays; the softer the rays the less ability they have to penetrate the human tissue. In penetrating the tissues they are dispersed, traveling in every direction, and by impact with the materials through which they pass new rays are given off of exactly the same degree of hardness as those which entered the tissue. The greater the bulk of the tissue radiated the greater the number of scatter rays produced. Friedrich has shown by measurements at depth that a considerable portion of these rays re-enforce the direct rays which penetrate to the point of measurement, therefore the amount of rays penetrating to a given depth is influenced by the size of the field at the surface. The amount of radiation at a given depth, say 10 cm., is expressed in percentage of the dose at the surface. This has been called, by Wintz, the percentage depth dose and represents the amount of radiation at 10 cm. depth in the tissue expressed in percentages of the skin dose.

The so-called carcinoma dose is taken at different clinics to be from 90 to 125% of the erythema dose. Such a percentage cannot be obtained through one field. Hence the proper dosage at depth is secured by exposure through multiple fields.

Accurate measurements on phantoms have been made to correspond with various sections of the human body. Professor Dessauer has prepared a series of fifty diagrams showing the exact distribution and possible values for different distances and different size fields.

On the whole the voltage, milliamperage, and filtration

remain approximately the same in the different clinics. The most frequent combination is with the Coolidge tube; 2 milliamperes on the tube, 30 cm. focal distance, 1 mm. copper filter, which with a field 10 cm. square will give an erythema dose in about 180 to 200 minutes.

In treating sarcoma it has been found that as high a dosage as the carcinoma dose is not required. In general about 60 to 70% of the skin dose is considered adequate.

In most German clinics the ideal to be attained is to give one full carcinoma dose and not to repeat the treatment until the full action of the radiation has spent itself. For this reason generally, divided dosage is not employed in the treatment of cancer with the exception of the Medical Clinic at Freiburg where divided dosage is still practiced.

It is realized that the reaction of the cells of a cancer is not always uniform, that certain cells are more easily affected by radiation than others. Furthermore that the reaction to the radiation is more prompt in some cells than others and that it is, therefore, necessary to wait a sufficient period of time before repeating treatment. In some instances the response of a tumor to radiation is as late as three months after treatment. Reference to a case of cancer of the uterus which first responded to radiation three months after treatment was given previously in the text.

Sarcomata frequently do not show evidence of regression until from 6 to 8 weeks after treatment. In the meantime the growth frequently increases in size; a very disconcerting phenomenon and one which has led to premature amputation in the past. The general procedure is giving of the full carcinoma dose at first radiation, repeated from 8 to 12 weeks, second pause of from 12 to 20 weeks and further pauses of 6 months.

DEEP THERAPY IN THE FIELD OF MEDICINE AND SURGERY OTHER THAN MALIGNANT DISEASE

The value of deep therapy to a general hospital is by no means confined to the value of this procedure in the treatment of neoplasms.

In the treatment of certain affections other than tumors, such as tuberculosis of the lungs, glandular tuberculosis, bone tuberculosis, leukemia and pseudoleukemia, exophthalmic goitre and to stimulate the healing of fractures or bone grafts, so-called stimulating dosage is used. This is a fraction of the erythema dose, ranging from 20 to 50% of the erythema dose. In this field divided dosage *is* employed. I was greatly struck by the extensive use made of deep therapy in the treatment of diseases other than malignancy.

Leukemia and Aleukemia

Leukemia and aleukemia are treated by radiating the spleen and the glands, small fields at intervals of five days, with 20% of the erythema dose.

Exophthalmic Goitre

Exophthalmic goitre is treated by small fields, interrupted dosage, usually two supraclavicular and two infraclavicular, using 20% of the erythema dosage. Not more than two radiations in one week so as to cover each field three or four times; the whole requiring from 6 to 8 weeks.

Pulmonary Tuberculosis

At the Medical Clinic in Freiburg, Professor Kuppferle showed me the records of a number of cases of pulmonary tuberculosis successfully treated by deep radiation. His practice is to treat only those of the productive type, especially cases where there is a tendency toward induration. After determining the character of the lesion with radiographs, cases of this type in which no more than one-third of both lungs is involved, after localizing the lesions and plotting the fields to be employed, one field is radiated at a time with approximately 5% of the erythema dose, allowing five-day pauses between each radiation.

Glandular Tuberculosis

Glandular tuberculosis responds remarkably well to deep therapy, 10% of the erythema dose being employed.

Bone and Joint Tuberculosis

Bone and joint tuberculosis is successfully treated by small stimulating doses repeated at intervals. Here the object to be attained is to stimulate the growth of the protective tissue engaged in a struggle to isolate the tubercle bacillus. If too large doses are given damage is done and cases of quiescent tuberculosis of joints and bones have been rendered acute in their progress and associated with development of miliary tuberculosis by overdosage. The result of small, divided stimulating doses is to soften and render more certain the progress to recovery. I inspected the histories of cases of advanced and hopeless joint tuberculosis, beyond the help of surgery which, under radiation, completely cleared up, leaving only such scar tissue and destruction of substance as were inoperable at the time of the first treatment.

Deep therapy is frequently used to soften scars and break up adhesions, in which field it has proved very successful, but at Freiburg I was told of cases of contracture and adhesion following clinically healed joint tuberculosis in which, when treated with large doses to soften the adhesions the protective tissue had broken down and the cases had relapsed.

Joint tuberculosis is a field in which heavy dosage for the removal of adhesions and contractures can not be employed. It is recognized that a dose of sufficient activity to destroy the tubercle bacillus in the tissue can not be given; furthermore, that a heavy dose which will break down granulation tissue and do damage, at the same time seems to stimulate the tubercle bacillus. The treatment of bone and joint tuberculosis seemed to me to be one of the most useful fields in which this procedure is employed.

Treatment of Fractures to Expedite Healing

This method was developed by Kohler of Freiburg. Attempts to stimulate the repair of fractures by the X-ray in the early days of radiography were made after it had been shown that the healing of the bones in small animals could be favorably influenced after fracture by radiation with the type of apparatus in use at that time. When it was attempted to secure the results upon human beings which had been attained with small animals the results were negative. In 1918 Kohler published 87 cases of fracture successfully stimulated with the X-ray. He attributes the present success to the improvement in the apparatus and the penetrating character of the new technique. The dosage used is approximately from 12 to 20% of the erythema dose. Most striking results are in adults; the rate of repair of young patients, under 15 years of age, being only slightly changed.

From this it is deduced that the radiation stimulates the possibilities of repair to the maximum. On the whole it may be said that the time of repair for fractures is cut in half, that the consolidation is more certain and better, and, that if the dosage is properly given and not overdone there is no risk. The treatment may be given through ordinary plaster dressings but it is often desirable to cut windows in the plaster case at the point where it is desired to give the treatment.

The success of bone grafts is greatly improved by similar radiation with stimulating dosage. At Lexer's Clinic in Freiburg these cases are always radiated; the dosage is similar to that for fractures.

DISCUSSION

Dr. Knight—Dr. Cook has kindly consented to discuss this subject of cancers.

Dr. Henry Wireman Cook—Mr. President and Gentlemen: I have read the full report and have again listened with the greatest interest to Dr. Gaylord's talk on Recent Develop-

ments in Cancer Therapy, especially that of X-ray and radium, and I feel that from the presentation, we are justified in hoping that present and later developments in X-ray treatment may offer hope of more encouraging results than are possible at this time.

However great interest there may be in this therapy from a medical standpoint, it does not appear that it has more than an indirect bearing on life insurance medical selection.

Here we are concerned with the cancer problem in two ways: first, the discovery of past or present existence of cancer at examinations; and second, the reduction of the present and very serious growing mortality from cancer.

In regard to the acceptance of applicants giving a history of cancer with successful treatment, a paper by Dr. Hunt of the Mayo Clinic at the March meeting of the Medical Section of the American Life Convention was interesting, but not encouraging to increased liberality beyond the very conservative attitude that has been held almost invariably by insurance companies. His summary was stated in these words:

> "Insurance may be offered with safety to many persons who have been treated for cancer of low or moderately low grade malignancy, when it exists as a localized process without regional glandular involvement, but inadvisedly if the cure has been of less than five years' standing."

When we take into consideration the difficulty of determining whether there is glandular involvement or not, and just what grade of malignancy exists—we can see that it will leave few cases which can be safely accepted even after five years, and that probably the only safe rule at present is that cases giving a history of cancer are not acceptable until after ten years have elapsed after operation or cure. As the majority of cases are over forty-five years of age, this would leave an almost negligible number for consideration at an insurable age. As so large a percentage of cancer in males is of the stomach—forty per cent. it would seem wise to select with

great care, or at an advanced rating, cases giving a history of indigestion or dyspepsia at forty-five or over.

The interesting and promising field in connection with cancer, for insurance companies, is the second phase of the subject—cancer control, and the reduction of the present alarming cancer death rate. Progress in this direction, until greater advance in treatment has been made, lies entirely in the education of the laity and the medical profession, to the end that cancer may be recognized and treatment sought before the case has passed the stage of probable response to any treatment.

In educational fields, life insurance companies occupy a position of the greatest advantage. Employing tens of thousands of physicians, and mailing to them once a month a welcome payment check, they have an incomparable avenue of approach.

However much value may attach to propaganda among the public at large, it is nevertheless true that a layman's opportunity for early diagnosis and treatment can only equal, never exceed, the knowledge of the physician he consults. If you have educated your layman to seek advice upon discovery of an ulcer or a lump or irregular bleeding, and the physician consulted makes light of it and says: "Come back in six months if it still bothers you," you have not favorably influenced the mortality rate for such a case.

Again, the most effective and authoritative source of medical advice is the physician—to his patient and to his community. Insurance companies can tactfully and forcefully instruct their examiners in the danger-signs of cancer, and can awaken them to a sense of their responsibility as teachers of preventive medicine. Much valuable information can be widely distributed to the public through an advised and interested agency force, and through papers and bulletins, by short and pithy dodgers enclosed to policyholders with company mail, and by seeing that one of the company medical staff has a paper on cancer control presented before the local medical society, and as many others as practicable. The American

Society for the Control of Cancer has a great deal of informative literature from which appropriate selection can be made.

This bulletin on cancer prevention was sent out to the medical examiners of the Northwestern National Life Insurance Company:

"The first week in November has been designated CANCER WEEK by the American Society for the Control of Cancer, and we take pleasure in directing the attention of our medical examiners to this inaugural effort in a more aggressive prophylactic campaign.

"Cancer is increasing as a mortality factor about two per cent. a year, and has reached the alarming total of 90,000 deaths per annum in this country alone.

"Bloodgood has recently stated that the *treatment* of cancer has not materially improved the mortality rate during the past three decades; the improvement in results has come from education. Practitioners of medicine and the laity are recognizing cancer earlier, and precancerous conditions more frequently. The improvement in the results of treatment is due to diagnosis and treatment before metastases have occurred. When favorable results can be expected in only 10 per cent. or less, after metastases have occurred, and cures in 50 to 60 per cent. before metastases,—the imperative need for educating both physicians and the laity is strikingly indicated.

"Most physicians are alert in recognizing cancer when the case is presented to them, but they have not kept in sufficiently close touch with the individuals of their clientele, nor have they sufficiently impressed upon their patients and all within the scope of their professional influence the initial danger signs of cancer, and the necessity for seeking immediate and competent medical advice.

"The great increase in cancer is shown by the figures from the State Board of Health of Minnesota, showing 1425 deaths from cancer in 1911, and 2285 deaths in 1920. The figures for the intervening years are as follows: 1911, 1425; 1912, 1473; 1913, 1625; 1914, 1703; 1915, 1780; 1916, 1955; 1917, 1936; 1918, 1890; 1919, 2054; 1920, 2285.

"Every medical man has a pressing responsibility far beyond the treatment of disease. Failure to share his knowledge regarding cancer with his patients and his community, means the sacrifice of many lives to a vicious malady. Many medical men who would consider themselves criminally negligent if they failed to properly examine and advise a patient who came to them complaining of a 'lump' or 'bleeding,' nevertheless do not sufficiently impress upon their patients and their communities the 'danger signals' of cancer, and the importance of periodic examination and immediate consultation when suspicious symptoms arise.

"The responsibility of the medical profession is not passive, and is not satisfied by leaving patients and the public in ignorance of the symptoms and signs upon the recognition of which the chance of a cure depends. This responsibility demands active steps for the education of the public. It is as great or a greater professional victory to educate a woman to recognize her malady in time for a cure, as to remove the growth successfully. The man who is referring or operating only late cases, is playing relatively but a minor part from a social as well as a medical point of view.

"Education in cancer control is the primary need, and the medical profession must fill the need."

In order to bring the subject of assisting in the cancer control program definitely before the meeting, I move you, Mr. President, that the Medical Directors' Association endorse the cancer control program, and that it recommend to constituent members such measures as may seem wise to protect the public health and the health of policyholders.

Motion was seconded by Dr. Rogers and carried.

Dr. Rowley—Mr. President, I know of no address that has been more interesting than this one by Dr. Gaylord. The subject of cancer is so big that when I found I was expected to take part in the discussion, I wondered on just what phase of it I was expected to talk. Then I thought that I would have

in advance an outline of Dr. Gaylord's address, so I would know how to fortify myself on some few points, but I did not have that advantage until I came here to the meeting. I will not therefore take up any time in dwelling on facts already well known to you. The point I would like to emphasize is just what Dr. Cook has spoken of—that is, the opportunity that is before life insurance companies particularly to take part in this campaign of education which has for its object the control and lessening of cancer. I am glad that Dr. Cook has put this motion before you and that you have acted upon it.

Motion was made and seconded and carried by a rising vote, that the Association of Life Insurance Medical Directors extend to Dr. Gaylord a vote of thanks for his kindness in coming down to New York, and making this address.

Dr. Knight—There remains the discussion of a very timely paper by Dr. Russell on "Albuminuria and Casts." This will take place after luncheon, and in the meantime we will go to the laboratory to see Dr. Exton's demonstration.

Immediately after luncheon Dr. G. A. Harlow presented the following report on the subject of goiter:

THE EXPERIENCE OF THE NORTHWESTERN MUTUAL LIFE INSURANCE COMPANY WITH SIMPLE GOITER AND WITH GOITER CASES AFTER OPERATION

By Dr. George A. Harlow

No one can study the goiter problem, as it exists to-day, without a feeling of optimism, if he believes that Kimball and Marine have discovered a workable method by which the in-

cidence of simple goiter can be measurably controlled. The figures of Smith (1), that 65,500 men, within a few years, passed through a physical examination at Jefferson Barracks, Missouri, and that 1074 of them showed an enlargement of the thyroid gland (1.63%), and that 116 men were rejected with a diagnosis of hyperthyroidism—exophthalmic goiter—show that goiter in the United States is much more prevalent than is generally supposed, and that the Great Lakes basin and the Cascade Mountain region of Oregon and Washington, do not by any means contain all the cases of mild goiter in this country. We are not, however, from a life insurance standpoint, particularly interested in the severer forms of the disease as manifested in myxœdema and cretinism, but the question that constantly confronts the medical directors of all life insurance companies is whether simple goiter cases can be accepted as satisfactory life insurance risks, and if accepted, on what basis they should be taken. There have been at least three divergent opinions on this question; First, that cases of simple goiter should not be accepted on any plan; Second, that all cases of simple goiter should be considered as substandard risks and rated up or an extra premium charged; Third, that a satisfactory group can be chosen from these cases of simple goiter that will give a favorable mortality, at standard rates.

It is easy for me to understand the attitude of the medical director who believes that all goiter cases should be rejected, because for more than half my life, I lived in a non-goiterous section of New England on the Atlantic seaboard, and rarely saw a case of goiter except in one of the big hospitals, where all goiters were looked upon as serious surgical or medical problems. Since residing for more than twenty years in one of the goiter zones, where mild cases of simple goiter are very common, I have come to modify my opinion as to the fatality of this disease. In the discussion of a very excellent paper, "Goiter in Relation to Life Insurance," read by Doctor Robert L. Rowley before this Association in 1917, (2) Doctor Rogers made the startling statement, "We have reason to

believe that there is a mortality of from 150 to 200% in goiters unoperated upon." In spite of those figures, the "radical" Northwestern has been placing on its books, at standard rates, cases of simple goiter, since June, 1909, believing that a very large percentage of these cases of simple goiter never develop toxic symptoms, and that those cases that do develop toxic symptoms, only do so after very many years. This opinion has been verified by a personal letter that came to me in February, 1921, from Doctor Henry S. Plummer of the Mayo Clinic. He said, "Simple goiter does not primarily increase the insurance risk. Simple and multiple adenomata of the thyroid usually start in adolescence. The lapse of time between the appearance of the goiter and hyperfunction is seventeen and a half years."

In Table I, you will note that we have accepted 779 cases of simple goiter without operation, from June, 1909 to October 1, 1921. Of this number, eleven have died:

1 of appendicitis
1 of diabetes mellitus
1 of gall-bladder disease
6 of influenza
2 of pulmonary tuberculosis

The average age at entry was 28 years; the average exposure, 3.74 years, with a mortality experience corresponding to the general average mortality of the Northwestern. It can be well said that this is too short a period for these cases to have been under observation, to say absolutely what the end mortality will be on this group, but at least we have a very significant straw pointing to an ultimate mortality record much better than might be feared.

Table II shows all the states from which the goiter cases have been selected. You will note that 332 members out of 779 were from Wisconsin.

Table III is a study of 117 cases of goiter accepted, after operation, from March, 1910 to October 1, 1921; age at entry,

33 years. Out of this group there has been only one death and that was from gallstones.

(1) *Journal American Medical Association* 1919, vol. lxxii., p. 471.
(2) *Association of Life Insurance Medical Directors of America*, 1917 to 1918, p. 120.

TABLE I

GOITER—WITHOUT OPERATION RISKS ACCEPTED NORTHWESTERN MUTUAL LIFE INSURANCE COMPANY MEDICAL DEPARTMENT

Issues of June, 1909 to October 1, 1921

Ages at Entry	Number of Members	Number of Deaths
16–19	99	1
20–24	231	5
25–29	169	1
30–34	116	3
35–39	83	1
40–60	81	0
Total.....	779	11

Average age at Entry 28 years
Average duration 3.74 years

CAUSES OF DEATH

	No.
Appendicitis	1
Diabetes mellitus	1
Gall-bladder disease	1
Influenza	6
Tuberculosis—pulmonary	2

TABLE II

GOITER PRESENT UPON EXAMINATION RISKS ACCEPTED NORTHWESTERN MUTUAL LIFE INSURANCE COMPANY MEDICAL DEPARTMENT

Issues of June, 1909 to October 1, 1921

Members Residing in the Following States

Wisconsin	332	Iowa	32	California	6
Illinois	71	Washington	29	North Dakota	5

Michigan	69	Pennsylvania	13	South Dakota	5
New York	44	West Virginia	9	Montana	4
Minnesota	42	Oregon	8	Nebraska	4
Ohio	40	Colorado	8	Idaho	3
Indiana	33	Missouri	7	Massachusetts	2

Thirteen miscellaneous states of one member each.

TABLE III

GOITER (STUDY) WITH OPERATION RISKS ACCEPTED NORTHWESTERN MUTUAL LIFE INSURANCE COMPANY, MEDICAL DEPARTMENT

Issues of March, 1910 to October 1, 1921

Ages at Entry	Number of Members	Number of Deaths
16–19	3	
20–24	16	
25–29	30	
30–34	25	
35–39	11	
40–60	32	1
Total.....	117	1

Average age at Entry	33 years
Average duration	3.05 years

CAUSE OF DEATH

	No.
Gallstones	1

Dr. Scadding—Mr. President, if you will permit me I would like to say a word to the Association on the subject of goiter. We have been insuring some school teachers for the Board of Education, and we have just examined 577 of them, 10% of whom showed goiter. I do not know whether 10% represents the true percentage of goiter in females, but I am inclined to

think it does not. Whether the occupation has anything to do with it, I do not know. There was a fair proportion of nervous breakdowns among these females, but ten per cent. seemed to me a rather large proportion of goiter. School teaching is thought to be a favorable occupation, and I suppose the reason for this thought is that so many of them do not really die as school teachers—they retire at 60 and we lose sight of them, and while I believe their lives are not very long they do live as a rule until sixty. The interesting point to me, however, is whether 10% represents the correct percentage of goiters among females. The average age was 39. I know there is a very large proportion of enlarged thyroids among young women in puberty, but among females of the average age of 39, this percentage seems large.

Dr. Eugene S. Russell then read the following paper:

ALBUMINURIA AND CASTS

By Eugene F. Russell, M.D.,

Medical Inspector, Mutual Life Insurance Company.

The significance of albumin and casts in the urine is very important from an insurance as well as a clinical standpoint. The mortality results in a large group of individuals are due to a greater or less degree to a proper understanding and interpretation of the clinical findings. During the past twenty years, many important contributions have been made to this subject, and it is interesting to trace our knowledge from the time when such abnormalities were considered of the most serious importance, to the present time when there are many who would perhaps lead us to believe that the presence of a trace of albumin, or a few casts, was desirable especially when indicative of some focus of disease; and which, if not eradicated, would lead to more serious complications.

Recently a very important addition has also been made to our knowledge of this subject, viz.: the Report of the Joint Committee of the Actuarial Society of America and the Association of Life Insurance Medical Directors on Intermittent Albuminuria. At the time the material for this report was compiled, the results of the Mutual Life were not available and, consequently, were not included. An investigation of this subject on applications issued in United States and Canada covering the issues of 1907 to 1917, exposures of 1917 to 1918, has recently been made and forms the basis of the following study and conclusions.

In every case where albumin was found on examination or albumin was believed to have been present within ten years, three specimens were examined—specific gravity 1010 to 1030. No case was accepted unless cleared by three normal specimens. No distinction has been made between "faint trace," "trace," "very faint trace," etc., as it is not believed that these findings alone influence mortality results. It is common knowledge that marked cases of nephritis may have a very faint trace of albumin one day, a large amount on the next or none at all.

For the purpose of this study we have analyzed our cases by various groups and have then summarized the results.

The symbols used are the same as those adopted by the Medico-Actuarial Committee.

TABLE I

MED. IMP. NO. 7-K (ALBUMIN FOUND ON EXAMINATION. CLEARED BY EXAMINATION OF THREE CONSECUTIVE SPECIMENS FREE FROM ALBUMIN.)

Ins. Yrs. 1–5

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29	623	1863	14	7.793	179.8
30–44	286	912	5	4.701	106.4
45 & over	112	327	4	4.089	97.8
Total....	1021	3102	23	16.583	138.7

		Ins. Yrs. 6 & Over			
15–29		340		1.657	
30–44		204	3	1.334	224.9
45 & over		57	2	1.055	189.6
Total....		601	5	4.046	123.5
		Ins. Yrs. All			
15–29		2203	14	9.450	148.1
30–44		1116	8	6.035	132.5
45 & over		384	6	5.144	116.8
Total....		3703	28	20.629	135.7

In this group there were 1021 entrants, giving a mortality of 135.7 for all insurance years.

An analysis of this table shows that the mortality ratio during the first five years of exposure was 179.8; for age group 15-29, an average of 148.1 for all insurance years; the average for age group 30–44, was 132.5 and for ages 45 and over, 116.8.

There were 28 deaths in this group, the causes of which are as follows:

TABLE II

Cause of Death	15 to 29	30 to 44	45 & Over
Septicæmia	1		
Tuberculosis of Lung	8		
Alcoholism	1		
Angina Pectoris		1	
Pneumonia		2	
Bright's Disease		2	
Cancer			1
Cirrhosis of Liver			1
Heart Disease	1		
Suicide	1	2	
Traumatism	2		1
Infl. of Brain		1	
Gen. Paralysis of Insane		1	
Apoplexy			1
Bronchitis			1
Total	14	9	5

A study of the causes of death shows the predominance of tuberculosis of the lungs in the younger ages. This cause of death accounted for 28.5% of the total number of deaths, and more than half the deaths among the entrants below 30.

Bright's disease accounted for 2 deaths only from 30 to 44.

TABLE III

MED. IMP. NO. 7-A (ALBUMIN FOUND WITHIN 2 YEARS, CLEARED BY EXAMINATION OF THREE CONSECUTIVE SPECIMENS FREE FROM ALBUMIN.)

Ages	Entrants	Expos.	Act.	Expected	Ratio
		Ins. Yrs. 1-5			
15-29	531	1635	6	6.879	87.2
30-44	381	1168	5	5.933	84.3
45 & over	160	481	4	6.761	59.2
Total....	1072	3284	15	19.573	76.7
		Ins. Yrs. 6 & Over			
15-29		207	1	1.015	98.5
30-44		208	4	1.398	286.1
45 & over		71	3	1.513	198.3
Total....		486	8	3.926	203.6
		Ins. Yrs. All			
15-29		1842	7	7.894	88.7
30-44		1376	9	7.331	122.8
45 & over		552	7	8.274	84.6
Total....		3770	23	23.499	97.9

An analysis of this table shows a very favorable mortality in the first five years of insurance in all age groups. In the second and third age groups the mortality is considerably higher than the normal in the later insurance years, but the average for all ages is comparatively satisfactory though 23 points higher than the general experience of the Company which was 75% by the M. A. Table.

There were 23 deaths which are distributed as follows:

TABLE IV

Cause of Death	15 to 29	30 to 44	45 & Over
Pneumonia	1	1	1
Bright's Disease		1	4
Typhoid Fever	1	2	
Tuberculosis of Lung	1		
Apoplexy	2		
Endocarditis		1	
Ulcer of Stomach		1	
Dis. of Stomach		1	
Biliary Calculi			1
Drowning	1		
Traumatism		2	
Sudden			1
Auto Accident	1		
Total	7	9	7

Tuberculosis of the lungs was the cause of only one death in this group although there were several acute and accidental causes which accounted for the high mortality in the age group 30–44. Bright's disease is evident as a cause of death in the older ages and was considerably above the expected.

TABLE V

Med. Imp. No. 7-B (Albumin Found Within 3 to 5 Years Cleared by Examination of Three Consecutive Specimens Free from Albumin.)

Ins. Yrs. 1–5

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29	225	626	4	2.640	151.5
30–44	204	643	7	3.280	213.4
45 & over	72	224	2	3.102	64.5
Total	501	1493	13	9.022	144.2

Ins. Yrs. 6 & Over					
15–29		112		.558	
30–44		162	1	1.105	90.5
45 & over		54	1	1.040	
Total....		328	2	2.703	37.0
Ins. Yrs. All					
15–29		738	4	3.198	125.1
30–44		805	8	4.385	182.2
45 & over		278	3	4.142	72.5
Total....		1821	15	11.725	127.9

It is noted that in this group the mortality ratio in the age group 15 to 29 was 151%, 30 to 44, 213% in the first five years. The average mortality for all the insurance years 15–29 and 30–44, was 125.1 and 182.2 respectively. Average total was 127.9.

The causes of death in this group are as follows:

TABLE VI

Cause of Death	15 to 29	30 to 44	45 & Over
Heart Disease..............			1
Suicide....................			2
Tuberculosis of Lung........	2	1	
General Disease.............	1		
Chronic Poisoning...........		2	
Apoplexy...................			1
Pneumonia..................		1	
Appendicitis................			1
Bright's Disease............		2	
Traumatism.................	1		
Total....................	4	6	5

Tuberculosis in the age group 15 to 29 accounted for 2 deaths. In the age group 30 to 44, Bright's disease likewise accounted for 2 deaths.

The following table gives the combined results where albumin was found on examination or once within five years of date of application without examination for casts. Cleared by examination of three consecutive specimens free from albumin.

TABLE VII

MED. IMP. NO. 7 (ALBUMIN IN THE URINE, WITHOUT EXAMINATION FOR CASTS.) SYMBOLS K, A AND B (FOUND ON EXAMINATION OR ONCE WITHIN 5 YRS. OF APPLICATION.)

Ins. Yrs. 1–5

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29	1379	4124	24	17.312	138.6
30–44	871	2723	17	13.914	122.2
45 & over	344	1032	10	13.952	71.7
Total......	2594	7879	51	45.178	112.8

Ins. Yrs. 6 & Over

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29		659	1	3.230	31.0
30–44		574	8	3.837	208.3
45 & over		182	6	3.608	166.2
Total......		1415	15	10.675	140.4

Ins. Yrs. All

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–19		4783	25	20.542	121.7
30–44		3297	25	17.751	140.9
45 & over		1214	16	17.560	91.1
Total......		9294	66	55.853	118.1

Where there were two findings of albumin, the last within two years, without albumin on examination, the following results were obtained:

TABLE VIII

MED. IMP. NO. 7 SYMBOL E (2 FINDINGS OF ALBUMIN, LAST WITHIN 2 YRS., CLEARED BY EXAMINATION OF THREE CONSECUTIVE SPECIMENS FREE FROM ALBUMIN.)

Ins. Yrs. 1–5

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–19	144	484	2	2.067	96.8
30–44	118	399		2.003	
45 & over	36	124	1	1.756	57.0
Total......	298	1007	3	5.826	51.5

Ins. Yrs. 6 & Over

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29		54		.265	
30–44		66	1	.427	234.2
45 & over		22	1	.408	245.1
Total......		142	2	1.100	181.8

Ins. Yrs. All

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29		538	2	2.332	85.8
30–44		465	1	2.430	41.2
45 & over		146	2	2.164	92.4
Total......		1149	5	6.926	72.2

In this group the mortality ratio for all insurance years was good, approximately three points better than the general experience of the Company, but the group was small and the deaths only 5 in all.

The causes were distributed as follows:—In age groups 15–29, typhoid and drowning, one death each; 30–44, disease of nervous system, one; 45 and over, tuberculosis, one; Bright's disease, one.

The following table shows the results where albumin was

found two or more times, the last within 3–5 years. Three consecutive specimens were examined and were free from albumin.

TABLE IX

MED. IMP. NO. 7—SYMBOL F (2 OR MORE FINDINGS, THE LAST WITHIN 3–5 YRS.)

Ages	Entrants	Expos.	Act.	Expected	Ratio
Ins. Yrs. 1–5					
15–29	39	93	1	.382	261.8
30–44	66	210	1	1.060	94.3
45 & over	7	24		.303	
Total......	112	327	2	1.745	114.6
Ins. Yrs. 6 & Over					
15–29		6		.030	
30–44		38		.232	
45 & over		6		.095	
Total......		50		.357	
Ins. Yrs. All					
15–29		99	1	.412	242.7
30–44		248	1	1.292	77.4
45 & over		30		.398	
Total......		377	2	2.102	95.1

In the group where there were two findings of albumin within three to five years, the number of entrants were also small and the one death from tuberculosis in the age group 15–29 was sufficient to make the ratio 261.8%. The other death in age group 30–44 was due to insanity.

A combination of Tables 8 and 9 gives the following results:

TABLE X

MED. IMP. NO. 7—SYMBOLS E AND F (2 OR MORE FINDINGS OF ALBUMIN, THE LAST ONE WITHIN 5 YEARS OF APPLICATION BUT NONE FOUND ON ORIGINAL EXAMINATION.)

Ins. Yrs. 1–5

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29	183	577	3	2.449	122.5
30–44	184	609	1	3.063	32.7
45 & over	43	148	1	2.059	48.6
Total....	410	1334	5	7.571	66.1
Ins. Yrs. 6 & Over					
15–29		60		.295	
30–44		104	1	.659	151.7
45 & over		28	1	.503	198.8
Total....		192	2	1.457	137.3
Ins. Yrs. All					
15–29		637	3	2.744	109.3
30–44		713	2	3.722	53.8
45 & over		176	2	2.562	78.1
Total....		1526	7	9.028	77.5

The following table gives the results where albumin was found over five years and within ten years of date of application. Three consecutive specimens examined free from albumin.

TABLE XI

MED. IMP. NO. 7—SYMBOLS C AND D (ALBUMIN FOUND WITHIN 6 TO 10 YEARS.)

Ins. Yrs. 1–5

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29	139	391	1	1.663	60.1
30–44	357	1053	3	5.365	55.9
45 & over	79	233		3.071	
Total....	575	1677	4	10.099	39.6

Ins. Yrs. 6 & Over

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29		62		.309	
30–44		200	1	1.382	72.4
45 & over		42	1	.925	108.1
Total....		304	2	2.616	76.5

Ins. Yrs. All

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29		453	1	1.972	50.7
30–44		1253	4	6.747	59.2
45 & over		275	1	3.996	25.0
Total....		1981	6	12.715	47.2

A study of this table shows the very favorable mortality in all age groups and for all insurance years. This is to be expected especially as the condition causing the albuminuria was either an acute or temporary condition and the kidney if injured has had sufficient opportunity to repair itself.

There were 6 deaths, the causes of which are as follows:—
15–29 —one death from heart disease.
30–44 —one insanity, 2 suicide, 1 traumatism.

45 & over—one heart disease.

A summary of the foregoing tables gives the following results:

TABLE XII

MED. IMP. NO. 7—ALL SYMBOLS

Ins. Yrs. 1–5

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29	1721	5,159	29	21.709	133.6
30–44	1474	4,562	23	23.191	99.2
45 & over	477	1,444	11	19.369	56.8
Total....	3672	11,165	63	64.269	98.0
Ins. Yrs. 6 & Over					
15–29		791	1	3.878	25.8
30–44		924	11	6.159	178.5
45 & over		262	8	5.149	155.4
Total....		1,977	20	15.186	131.7
Ins. Yrs. All					
15–29		5,950	30	25.587	117.2
30–44		5,486	34	29.350	115.8
45 & over		1,706	19	24.518	77.5
Total....		13,142	83	79.455	104.4

This shows the high ratio in the first age group in the first five years of insurance and the high ratio in the older ages in the later insurance years.

A summary of the causes of death is given in the following table:

TABLE XIII

Cause of Death	15 to 29	30 to 44	45 & Over
Typhoid Fever	2	2	
Septicæmia	1		
Tuberculosis of Lung	12	1	1
Cancer			1
Alcoholism	1		
Gen. Paralysis of Insane		3	
Heart Disease	2		2
Angina Pectoris		1	
Pneumonia	1	4	2
Cirrhosis of Liver			1
Bright,s Disease		5	5
Suicide	1	7	
General Disease	1		
Chronic Poisoning		2	
Inflam. of Brain		1	
Apoplexy	2		2
Dis. Nervous System		1	
Endocarditis		1	
Bronchitis			1
Ulcer of Stomach		1	
Dis. of Stomach		1	
Appendicitis		1	1
Biliary Calculi			1
Dis. of Digestive System	1		
Drowning	2		
Traumatism	3	3	1
Sudden			1
Auto Accident	1		
Total	30	34	19

It may not be out of place for the purpose of comparison to quote the report of the Medico-Actuarial Committee on intermittent albuminuria, as follows:—

Ins. Yrs.	Actual Deaths	Expected Deaths by M.-A. Table	Ratio
1–5	35	34.6	101%
6–10	17	12.5	136%
11 & over	6	6.2	97%
Total.....	58	53.3	109%
Ages at Entry			
15–29	23	27.2	85%
30–44	22	18.9	116%
45 & over	13	7.2	181%
Total.....	58	53.3	109%

In this group there were 2661 entrants as against our group of 3672. The mortality in the younger ages was 85%—our group 117.2. The second age group was 116%, which compared with the Mutual Life experience of 115.8. The third age group of the Committee was exceedingly high as compared with ours of 77.5. This may be accounted for by the fact that there were possibly a number of cases of true kidney disease in the group.

In view of the very favorable mortality ratio as shown by Table 11 where albumin had been found over five years previous to examination, this group of cases were excluded and analysis of the cases showing albumin once or twice within five years, was made with the following results:—

TABLE XIV

MED. IMP. NO. 7, SYMBOLS K, A, B, E AND F COMBINED

Ins. Yrs. 1–5

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29	1562	4,701	27	19.761	136.6
30–44	1055	3,332	18	16.977	106.0
45 & over	387	1,180	11	16.011	68.7
Total...	3004	9,213	56	52.749	106.3

Ins. Yrs. 6 & Over

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29		719	1	3.525	28.3
30–44		678	9	4.496	200.0
45 & over		210	7	4.111	170.3
Total...		1,607	17	12.132	140.1

Ins. Yrs. All

Ages	Expos.	Act.	Expected	Ratio	General Experience of Mutual Life for Same Issues
					Ratio
15–29	5,420	28	23.286	120.2	74.6
30–44	4,010	27	21.473	125.8	73.8
45 & over	1,390	18	20.122	89.5	75.8
Total...	10,820	73	64.881	112.5	74.8

This also shows the high ratio in the younger ages, and is much higher than the general experience of the company as is shown by the comparative table.

Referring to table 13 it is noted that tuberculosis and Bright's disease are the most prevailing causes of death. A comparison of the causes of these deaths with the general ex-

perience of the Company and the ratio per 10,000 was made with the following results—

TABLE XV

DEATHS FROM TUBERCULOSIS AND BRIGHT'S DISEASE WITH THE RATIO PER 10 M. SYMBOLS K, A, B, E AND F

	Deaths				*Ratio Per 10 M*				*Gen'l. Exper. of Co.*			
	15 to 29	30 to 44	45 & Over	Total	15 to 29	30 to 44	45 & Over	Total	15 to 29	30 to 44	45 & Over	Total
Tuberculosis......	12	1	1	14	22.1	2.5	7.2	12.9	5.5	4.4	4.5	4.9
Bright's Disease...		5	5	10		12.5	36.0	9.2	.7	2.4	9.6	2.7

Average Dur. Tuberculosis 3.5—Bright's Disease 5.6.

The total number of deaths from tuberculosis was 14. The ratio per 10,000 is 12.9% while the general experience of the Company was 4.9%. In other words, the deaths from tuberculosis were almost three times the general experience of the Company. Bright's disease accounted for 10 deaths. The general ratio per 10,000 was 9.2% while the general experience was 2.7%, accounting for three times the general experience of the Company. The average duration of insurance, with tuberculosis was 3.5 years; Bright's Disease 5.6 years.

In view of the fact that tuberculosis and Bright's disease were the most common causes of death, and as tuberculosis accounts for a good percentage of the deaths in the light weight group and Bright's disease in the heavy weight groups, an analysis of the build groups was made.

Table 16 gives the ratio in the light weight group, Symbols K, A & B only are included as there were too few cases in the other groups to be of any value.

TABLE XVI

CLASS NO. 7, ALBUMIN IN THE URINE, FOUND ON EXAMINATION OR IN ONE SPECIMEN WITHIN 5 YEARS. SYMBOLS K, A AND B, COMBINED. LIGHT WEIGHT GROUP, BUILD 7, 8 AND 9

Ages	Entrants	Expos.	Act.	Expected	Ratio
Ins. Yrs. 1–5					
15–29	363	1111	7	4.651	150.6
30–44	98	317	3	1.563	191.9
45 & over	27	92	1	1.051	95.2
Total....	488	1520	11	7.265	151.4
Ins. Yrs. 6 & Over					
15–29		179	1	.877	114.0
30–44		71		.457	
45 & over		9		.115	
Total....		259	1	1.449	69.0
Ins. Yrs. All					
15–29		1290	8	5.528	144.6
30–44		388	3	2.020	148.5
45 & over		101	1	1.166	85.8
Total....		1779	12	8.714	137.8

Twelve deaths in this group were distributed as follows:—

15–29	Tuberculosis 4; Apoplexy 1; Heart Dis. 1; Traumatism 2.
30–44	Insanity 1; Traumatism 2.
45 & over	Bright's disease 1.

The medium weight group showed a comparatively better mortality ratio, as is shown by table 17, but tuberculosis is still a very important factor in the early ages, while Bright's disease accounts for as many deaths in the latter age periods. The results are as follows:—

TABLE XVII

CLASS NO. 7, ALBUMIN IN THE URINE, SYMBOLS K, A AND B, COMBINED. MEDIUM WEIGHT GROUP, BUILD 6, 0 AND 1

Ages	Entrants	Expos.	Act.	Expected	Ratio
Ins. Yrs. 1–5					
15–29	985	2909	17	12.213	139.2
30–44	631	1967	11	10.070	109.2
45 & over	244	704	5	9.499	52.7
Total....	1860	5580	33	31.782	103.9
Ins. Yrs. 6 & Over					
15–29		469		2.295	
30–44		399	7	2.643	264.9
45 & over		117	5	2.187	228.6
Total....		985	12	7.125	168.4
Ins. Yrs. All					
15–29		3378	17	14.508	117.2
30–44		2366	18	12.713	141.6
45 & over		821	10	11.686	85.5
Total....		6565	45	38.907	115.7

There were 45 deaths in this group, distributed as follows:—

TABLE XVIII

CAUSE OF DEATH	15 to 29	30 to 44	45 & Over
Typhoid Fever	1	2	
Tuberculosis	7	1	
Cancer			1
Alcoholism	1		
Other Gen. Dis.	2	1	
Apoplexy	1		1
Other Nervous Dis.		1	
Angina Pectoris		1	

TABLE XVIII (*Continued*)

Cause of Death	15 to 29	30 to 44	45 & Over
Pneumonia	1	2	2
Disease of Liver			1
Other Digestive Dis.		2	
Bright's Disease		4	3
Suicide	1	4	
Drowning	1		
Traumatism	1		1
Sudden			1
Auto Accident	1		
Total	17	18	10

The heavy weight group as a whole did not show as high a mortality as the light weight group. This may be due to the small number of entrants and a more rigid selection of the cases.

The results are given in the following table.

TABLE XIX

Class No. 7, Albumin in the Urine, Symbols K, A and B, Combined. Heavy Weight Group, Build 2, 3, 4 and 5

Ins. Yrs. 1–5

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29	31	104		.448	
30–44	142	439	3	2.280	131.6
45 & over	72	231	4	3.283	122.0
Total	245	774	7	6.011	116.5

Ins. Yrs. 6 & Over

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29		11		.054	
30–44		104	1	.739	135.3
45 & over		54	1	1.241	80.6
Total		169	2	2.034	98.3

		Ins. Yrs. All			
15–29		115		.502	
30–44		543	4	3.019	132.5
45 & over		285	5	4.524	110.6
Total ..		943	9	8.045	111.8

There were nine deaths in all. None occurred in the age group 15–29. Four deaths in group 30–44 were distributed as follows:—one cause not defined; one heart disease; one pneumonia; one Bright's disease. In the group 45 and over, there were five deaths; one apoplexy; one angina pectoris; one pneumonia; one disease of liver; one appendicitis.

A summary of the average age at death and the average duration of the policies from tuberculosis and Bright's disease in the light, medium and heavy weight groups, is given in the following table.

TABLE XX

Weight Groups	*Tuberculosis*		*Bright's Disease*	
	Average Age at Death	Average Dur.	Average Age at Death	Average Dur.
Light Weight....	26.5	5.0	49.0	4.0
Medium " ...	27.4	3.0	52.6	5.6
Heavy " ...			41.0	6.0

In the light weight group for ages 15 to 29 there were four deaths from tuberculosis making a ratio of 31 per 10,000. In the medium weight group for 15–29 there were 7 deaths from tuberculosis, a ratio of 20.7 per 10,000. In the heavy weight group there were no deaths from tuberculosis.

There was one death from Bright's disease among the light weights in the age group 45 and over, which gave a mortality ratio of 99 per 10,000. In the medium weight group, 4 deaths occurred for the age group 30–44 a ratio of 16.9 per 10,000, 13 for the age group 45 and over a ratio of 36.5 per 10,000.

In the heavy weight group there was only one death and

that occurred in the age group 30–44 a ratio of 18.4 per 10,000.

Summarizing the foregoing date, it is evident from our results that intermittent albuminuria in the younger ages cannot be entirely disregarded. In this conclusion it will be necessary to differ from the report of the Special Committee "that intermittent albuminuria is of comparatively little moment at the younger ages."

That tuberculosis plays an important part in this group of cases cannot be doubted. Whether the albumin is an indication of the focus of infection in the lungs or an indication of lowered resistance on the part of the individual, which renders him more susceptible to infection, is a question which at the present time cannot be answered to our complete satisfaction.

In the later ages, nephritis plays an important factor as the cause of death. This coincides with the opinion of the special committee. The conclusion "that there may have been included among the cases of intermittent albuminuria some cases of real nephritis" is evidently correct.

Nucleo-Proteid

Closely allied to albumin in the urine is nucleo-proteid. Nucleo-proteid is usually considered as due to the secretion of proteid from the urinary tract and of no significance. For several years past we have kept a record of these cases and the following tables represent our experience and the causes of death.

TABLE XXI

ISSUES OF 1907 TO 1917, EXPOSED TO 1907–1918
EXTRA NO. CODE II—NUCLEO-PROTEID PRESENT ON EXAMINATION
INS. YRS. 1–5

Ages	Entrants	Expos.	Act.	Expected	Ratio
15–29	531	1359	7	5.630	124.3
30–44	752	1969	8	10.131	79.0
46 & over	456	1184	17	15.240	111.6
Total....	1739	4512	32	31.001	103.2

INS. YRS. 6 & OVER

15–29		197	2	.967	206.8
30–44		317	1	2.264	44.2
45 & over		164	3	3.381	88.8
Total....		678	6	6.612	90.8

INS. YRS. ALL

15–29		1556	9	6.597	136.4
30–44		2286	9	12.395	72.6
45 & over		1348	20	18.621	107.4
Total....		5190	38	37.613	101.1

TABLE XXII

Cause of Death	15 to 29	30 to 44	45 & over
Tuberculosis of Lung........	4		1
Syphilis....................		1	
Cancer......................		2	3
Diabetes....................			1
Alcoholism..................	1		
Apoplexy....................			1
Angina Pectoris.............			1
Broncho-Pneumonia...........			1
Pneumonia...................		1	3
Ulcer of Stomach............		1	1
Bright's Disease............		3	6
Burns.......................	1		
Traumatism..................		1	1
External Violence...........			1
Auto Accident...............	3		
Total.......................	9	9	20

An analysis of these tables shows the high mortality ratio among the earlier ages, accounted for by tuberculosis. In the later ages, Bright's disease accounts for a number of deaths. Whether nucleo-proteid has obscured the detection of serum albumin or was mistaken for serum albumin cannot be definitely decided at present, but it is evident, however, that the finding cannot be totally disregarded and should always lead to a more thorough and careful examination of the urine.

Hyaline and Granular Casts

The number of cases entering into this group were too small to give any data of practical value. The mortality ratio, however, in this group was considerably higher than the expected.

A study of the mortality experience of cases declined by the Company for additional insurance on account of hyaline casts found on examination during the years 1902–1917, exposed to 1920, gives the following results. The only impairment which caused the rejection of these cases was the presence of hyaline casts. No other impairment, either in the urine or elsewhere, was taken, so that the group represents a number of applicants who were rejected solely for hyaline casts. Most of them had previous insurance which enabled us to obtain their subsequent history. The following tables give the results.

TABLE XXIII

THE RESULTS OF CASES DECLINED BY COMPANY FOR HYALINE CASTS WITHOUT ANY OTHER IMPAIRMENT

Expected Based on the M. A. Table

Ages	Entrants	Expos.	Act.	Expected	Ratio	Aver.
25–29	1	8		.037		
30–34	3	44		.259		150
35–39	1	15		.111		
40–44	7	58		.480		
45–49	12	90	3	1.101	272.5	
50–54	4	35	1	.622	160.8	
55–59	15	134	5	4.338	115.2	
60–64	5	23	3	.841	356.7	
65–69	1	6		.334		
Total	49	413	12	8.123	147.8	
Under 45	12	125		.887		
Over 45	37	288	12	7.236	165.7	

TABLE XXIV

A Study of the Causes of Death, Age at Death and Duration of Policies of the Actual Deaths

Cause of Death	Age	Duration
Typhoid fever	64	8
Cancer	62	5
Heart Disease	53	13
" "	56	10
" "	64	1
Pneumonia	48	5
"	57	14
Asthma	58	8
Appendicitis	47	5
Cirrhosis of liver	57	14
Nephritis & Arteriosclerosis	58	13
Unknown	45	4

It is noted that heart disease accounted for three deaths whereas nephritis accounted for only one. It is very evident that where there is an involvement of the kidney, it is almost impossible to foretell accurately what may happen to the heart or blood vessels.

From the experience of the Mutual Life it will be seen that albumin and casts represent distinct impairments, depending upon the age at entry, the duration and amount of impairment.

Dr. Rogers in his paper read before the Actuarial Society in 1917, stated that cases which showed only a trace of albumin in the first of several specimens, examined at different times, may be looked upon as free from impairment, provided there is no previous history and the risks are first class in other respects. This does not seem to have been the experience of the Mutual Life. He also states that cases which show albumin on more than one occasion may not be regarded as standard risks, except in possibly the very young, and then only if they are in other respects superstandard risks. In addition to this it may also be said that the seriousness of the

impairment depends upon how intensively the individual case has been studied. The high mortality experience in this group of cases depends not entirely on the involvement of the kidney but also somewhat on the fact that in cases of nephritis, the resistance to infection is very much lessened.

To diagnose chronic nephritis in its incipient state is becoming more and more important, as a larger group of individuals apply for larger amounts of insurance. To interpret urinary findings in terms of mortality is a problem which is solved only by the most extensive study.

As experimental medicine advances, it is becoming more evident that where the kidney is concerned, we are dealing with a very complicated organ and that it is impossible to determine the extent of the lesion by the amount of albumin and casts; that in disease only a small portion of the kidney may be involved by the process with the renal function continuing undisturbed. Hence by our present clinical methods it is only possible to judge in a general way the extent of the lesion. The modern theory is that albumin is due either to a leakage through the glomerulus or a secretion of the tubules and that casts are formed in the tubules by the absorption of protein and water from the filtrate. When the tubules are involved in the process, the casts may have granules, red blood cells, white blood cells, etc., attached to them or free in the urine. If we accept these theories we may be able to explain and judge to a certain degree the extent of the lesion.

If we exclude (1) the so-called cases of albuminuria of adolescence or albumin without known cause, which cases are becoming fewer and which should only be acceptable for insurance after the most careful examination and exclusion of any possible source of infection, and (2) those cases of evident nephritis which are not acceptable except only on prohibitive rates, and confine our remarks to that class of risks which show a few hyaline casts on examination (and which every Medical Director is familiar with) our problem resolves itself into the determination of (1) whether the kidney is damaged, (2) the amount of damage already inflicted, (3) the possibility of re-

moving the source of irritation, and (4) the ability of the kidney to carry on its work after the removal of the irritation.

To be able to form a concise opinion of the above factors we must rely not only on the chemical and microscopical examination of the urine but on associated signs and history.

As has been stated, where there is a degeneration of the cells of the tubules, there may be present red blood cells, pus cells, or granules on or in the body of the cast or free in the urine. Therefore, in judging the extent of the kidney lesion, it is important that we take into consideration the presence of these associated elements in the urine.

The amount of albumin and the number of casts may vary in the most marked case of chronic nephritis and it is even possible to have the urine free from albumin and casts in such a case. So in making a prognosis it is imperative that more than one specimen of urine should be studied.

A fixed low specific gravity of the urine indicating the inability of the kidney to concentrate is a very important factor and, according to Cabot, is more important than the examination of the blood for retention products.

A previous history of scarlet fever, diphtheria, etc., or the presence of a focus of infection, should always be taken into account in these cases. Likewise a history of symptoms referable to the kidney, such as polyuria, pain, etc., must be considered. Cabot especially emphasizes the presence of nocturnal polyuria as being one of the most important and early signs of disease. Prostatic trouble, of course, should be eliminated.

Urea determination is of small significance unless we can get twenty-four hour specimens and know the patient's diet. As a means of checking up the specific gravity in a specimen where preservation has been used, it is of value.

Blood pressure readings are likewise important, as one of the first signs of kidney involvement is the increase in the systolic or diastolic blood pressure. Whether the blood pressure is the cause of the kidney lesion or the result is not material from our standpoint.

Another most important procedure in this type of cases is the examination of the eye grounds by the ophthalmoscope as a means of detecting chronic nephritis. It is stated by some authorities that from 7 to 30% of cases of nephritis have an involvement of eye grounds.

Functional tests and blood chemistry will also give important data but, unfortunately, cannot be generally employed. Where a large amount of insurance is involved they should be insisted upon if the urinary impairment indicates such a necessity. As an indication of early disease, these tests are not of much value, according to the best authorities, but in late cases of nephritis associated with the increased blood pressure they are of the greatest importance. An examination of the blood which shows retention products is an important factor in giving a prognosis but a negative blood test must not be relied on to too great an extent—and should only be considered in conjunction with the other findings. This same statement holds true also of function, concentration and diet tests.

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DISCUSSION

Dr. Knight—Dr. Daley will open the discussion of Dr. Russell's paper.

Dr. Daley—Owing to the very short time which has elapsed since this paper was assigned to me, it has been impossible

to give it proper and analytical study and the consequent discussion which it deserves.

I believe that all the Medical Directors here present will agree that the presence of albumin in the urine indicates some disturbance of bodily function, and in a great many cases this disturbance is not due to organic disease of the kidneys. Consequently, in a class of cases showing the presence of albumin, we would expect to find a higher mortality than average, and that the cause of these extra deaths would not necessarily show an undue prominence of kidney disease.

With a history of albumin in the past—not present on examination—we should expect either one of two things: First, that the disturbance of bodily function which caused the albumin had been rectified or, secondly, that the kidneys had acquired a tolerance to the abnormal bodily constituents that caused their irritation and no longer excreted albumin, although the cause of their previous irritation still existed.

I was not in a position to submit a table of the Equitable's experience on cases that gave a history of albumin in the past, not found on examination. An investigation which was made showed that the Equitable experience with similar risks coincides with the M.-A. experience—low mortality at young ages of entry, higher mortality at older ages. But is the Mutual Life experience and our experience well comparable? Our material for the experience excludes any risks that are otherwise not A1. Else if the mortality prove high, we should not know whether the high mortality was due to a lot of lightweights, or heavyweights, or to impaired family history, etc. The Mutual Life material may contain borderlike risks with other minor impairments not bad enough to exclude them from the standard class.

The experience of the Equitable in its substandard class (albumin found on examination) based on the mortality experience of 1900 to 1916, issued between the years of 1900 and 1915, using the Equitable's Select and Ultimate Mortality Table, which deviates but little from the M. A. Table, is as follows:

IMPAIRMENT GROUPS

U. S. Rate Policies, Excepting U. S. South

	Actual Loss	Mortality Per Cent
Trace of albumin constant	31 (5)*	102.3%
Small amount of albumin constant	20 (1)	121.7
Large " " " "	1	102.3
Trace, Small and Large Albumin Interm.	4 (1)	184.6
Amount of albumin not stated	6 (1)	231.2
	62 (8)	117.9%

Albumin Impairments by Years of Exposure and by Age Groups

Years of (1–5)	32 (3)	128.4
Exposure (6–16)	30 (5)	107.8
Ages to 39	47 (4)	113.0
Age 40 and over	15 (4)	133.0

*31 policies, 26 lives

While this class is not as large as we should like to have it, owing to the elimination of cases showing other impairments, it will be seen that the mortality experience is somewhat more favorable than given in Dr. Russell's Table No. 1. From this it would seem that it is better to have albumin than to have had it, and I believe the error which exists here is due to the small exposure.

CASES IN WHICH THE ONLY IMPAIRMENT CONSISTS OF THE PRESENCE OF HYALINE CASTS.

A short analysis of these cases was made in 1919, with the following result (they were divided into four classes):

1. Those taken as standard risks.
2. Those taken as substandard and policies paid for.
3. Those taken as substandard but the policies N T O. Such cases of course became a subject of this investi-

gation only if the Equitable had other policies in force on the same lives.

4. Declined cases, which also could enter into this investigation only if other policies were in force.

GROUPS AT AGES OF ENTRY

	–29	30–39	40–49	50 +	All Ages	Mortality in % of Equitable Select and Ultimate
Standard Policies......	.542	*2* 2.516	*4* 3.233	*9 (4)* 7.526	*15 (4)* 13.817	93%
Subst. Pols. issued.....	.502	*3* 2.178	*5 (1)* 3.368	*3* 4.389	*11 (1)* 10.437	110%
" " N T O....	.230	*1* .831	*1* 2.013	*1* 3.514	*3* 6.588	52%
Declined............		.192	*1* .399	*8* 7.080	*9* 7.671	133%
	0 1.274	*6* 5.717	*11 (1)* 9.013	*21 (4)* 22.509	*38 (5)* 38.513	99%

GROUPS OF POLICY YEARS

	1st	2nd	3–5th	6–10th	11 +	All Years	Mortality
Standard Pols.....	*2 (1)* 1.088	*2* 1.204	*2* 3.940	*3* 5.612	*6 (3)* 1.973	*15 (4)* 13.817	93%
Subst. Pols. issued.	.693	*3 (1)* .626	*2* 2.329	*5* 4.029	*1* 2.760	*11 (1)* 10.437	110%
Subst. Pols. N T O.	.412	*1* .482	1.850	*1* 2.940	*1* .904	*3* 6.588	52%
Declined.........	.548	*1* .727	*3* 2.448	*4* 3.018	*1* .930	*9* 7.671	133%
	2 (1) 2.741	*7 (1)* 3.039	*7* 10.567	*13* 15.599	*9 (3)* 6.567	*38 (5)* 38.513	99%

Black figures in the line are the expected losses, the red figures above the line give the number of deaths. The numbers in

brackets after the figures of the number of deaths give the excess of policies over lives. Thus 8 (3) means 8 policies carried by 5 lives. The Mortality is expressed in per cent. of the Expected by the Equitable Select and Ultimate (1899-1909), the actual deaths being adjusted on the assumption that 9/10 of the excess policies over lives are distributed proportionally to the deceased in each of the four classes, while the other 1/10 of the excess policies over lives is charged to the class in which the excess happens.

The standard policies with a 93% mortality proved to be better, as they should be, than the substandard or the declined.

The substandard policies give a mortality of only 110%. A number of these substandard policies were later on, upon reëxamination, changed to standard policies, but were left in the group in which they started.

The applicants who refused to take substandard policies give a much lower mortality (52%). If this death rate were backed by a larger number of exposures, we could take it as a proof that applicants refused rated policies because they considered themselves standard risks, able to secure standard policies in other good companies.

The declined applicants, nearly all of whom were 50 years old, or older, show the highest mortality, yet at that only 133% of the Equitable Standard Table.

CAUSES OF DEATH

Phthisis	2	Pulmonary	2
Cancer	3	Digestion	3
Diabetes	1	Nephritis	3
Nerve Disease	2	Bone Disease	1
Apoplexy	3	Suicides	3
Circulatory	4	Accidents	2
Arteriosclerosis	5		

This is a comparatively small class and shows a much more favorable experience than that of the Mutual. It might well be deduced that from a medical selection point of view, the presence of hyaline casts might be disregarded.

Dr. Hoffmann, of the Prudential, once told me that anything could be proved by statistics, meaning that one should look carefully into the material from which they are made and carefully analyze the steps that lead up to the general conclusion before final acceptance. Let us, therefore, make just a short review of these figures before final acceptance.

First, as to the material, take, for instance, those cases accepted at standard rates. They were mostly cases where hyaline casts were found intermittently, one or two in the sediment of ½ ounce of urine, with a preponderance of absolutely normal specimens; these examinations being made in the greater part by the carefully conducted laboratory of the Equitable, as you have seen by the paper of our chemist, Mr. Wolf, last year. Almost any Company would have accepted them, yet you will notice that in the later years, after medical selection had worn off that the mortality began to rise. In the substandard class the reverse appears to be true, and would confound the above argument. But note the very good mortality among those who were offered substandard policies and would not take them. The Equitable experienced the highest selection at the earlier ages of issue because of adverse personal selection. Those who felt something was wrong with them took the policies offered. Those who were satisfied that they were good healthy individuals did not take them. Substandard business must be judged from a different angle than standard business. Review the causes of death, mostly chronic diseases and elusive from a viewpoint of medical selection. So while it would first appear that the Equitable has had a favorable experience with policyholders who had a more or less number of hyaline casts at the time of examination, a cursory analysis of the figures tends to show that it may not ultimately result as well as the figures would indicate. In the writer's opinion, the mortality to be expected from a class of risks whose only impairment is casts found constantly is somewhat more than the mean $\pm$ age between these figures and those of the Mutual as presented by Dr. Russell.

Dr. Russell, in comparing the deaths from tuberculosis has,

I fear, used statistics. He compares the deaths of the classes he presents with the general experience of the Company. I cannot understand how our beloved Dr. Symonds, whose actuarial paper last year is looked upon with awe by all us Doctors, let him commit such a fallacy. Every insurance Company of long standing, such as the venerable Mutual, has a higher death rate from cancer than tuberculosis, but the former preponderates in the policies of over 10 years in force; the latter among the policies of less than 10 years in force. (The younger ages.) Take the Equitable's experience among the latter.

4489 deaths from issues 1907 to 1917 were examined—all the deaths of these issues occurring from 1907 to 1915; stopping with 1915 because the war deaths began to count. Among those 4489 deaths (all ages at entry) just 10% were due to tuberculosis of the lungs. Taking ages at entry up to 30 only, the deaths from tuberculosis of the lungs were just 20%. These ratios from phthisis were lower but only slightly lower than the deaths from phthisis in the history of the albuminuria class. In this class they were 16% and 22% respectively. But the number of deaths in this class were very few, only 5, so that we cannot place much dependence on them. The Mutual experience says their result shows a death rate from phthisis of 3 or 4 times the ordinary.

In closing, may I say that albumin, constant, in the urine appears to indicate that there is functional dyscrasia, which may be due to subacute condition outside of the kidney; casts, persistent, in the urine indicate a chronic organic disease of some part of the body.

Dr. Symonds—I hate to stick a pin in that balloon of Dr. Daley's but the truth of the matter is that those figures which were given as the general experience of the Mutual Life was the general experience of the same issues as those upon which the albuminuria experience was given—that is, the issues of 1907–1917, exposed to 1918.

Now I want to say a word or two about this paper, because when the Joint Committee of Medical Directors and Actuaries prepared their report on intermittent albuminuria, the Mutual

Life made no contribution to their material, simply for the reason that we had nothing that the Committee called intermittent albuminuria. Each one of our cases was cleared up by three consecutive clean specimens in our laboratory. You will remember in the Joint Committee's report that the utmost they allowed were two consecutive clean specimens, so we did not contribute to that material at all. Some of our cases have had even a larger number of examinations, but all have had at least three consecutive clean specimens, and when the Committee's report came out I agreed with it quite as well as I did with anything the Committee furnished. As I have a good deal of belief in that Committee, I thought I would take an analysis of our own results, and to my horror found that our accidental albuminurias were very bad. The number is not very great and the loss ratio was better distinctly than the death ratio, but the results were bad, and bad right in the ages where the Committee said that accidental albuminuria could be ignored, and even intermittent albuminuria could be passed over. For that reason it seemed to me to be a very appropriate matter that they should be brought to the attention of the Association. It is contrary to the findings as expressed in our Presidential address yesterday morning. In that case I think that the findings were based upon rejected business; and in Dr. Daley's case on substandard business. Again, we had the faith that with three clean specimens we could ignore it, and therefore the cases were accepted fairly freely, but with that freedom we got two distinct results; tuberculosis, especially in the young, and to some degree in the middle ages, and Bright's in the middle ages and later. Those two facts stood out very distinctly, and I think they are the things we have to look out for in connection with our albuminurias. One finding of albumin is a danger flag that we have to consider and watch, and I think if you will ask Dr. Dwight he will have something to say on that subject. Of course we have modified our procedure in connection with these risks, and I hope we will get better results, but this was something to bring to the Association to correct

the effect of the Committee's report of a year and a half ago.

Dr. Knight—We would be very glad indeed to hear from Dr. Dwight.

Dr. Dwight—Mr. President, I had not intended to say anything but Dr. Russell asked me yesterday if I would not say a few words. I am very glad to.

For the benefit of the younger members, I would say that eighteen years ago we were very much in the same position that most companies are at the present time. We made a prolonged and serious study and arrived at two conclusions. First,—that while albumin was not a normal constituent of the urine, it was found in the urine of normal individuals to such an extent that many of such cases might be accepted. We established an experimental method of selection which, for Dr. Eakins's benefit, I would say was not scientific but, when measured by his yardstick, was evidently practical, because it reduced the percentage of our declinations and gave us a satisfactory mortality which has persisted ever since.

I have not made any investigation during the last four years, I think since Dr. Blakely read a paper here, but at no time has the mortality on business which was accepted with a history of albumin or albumin with casts equalled the general mortality of the company. So our results have been eminently satisfactory to us.

I would like to speak of two or three points in connection with this very good paper which I should like either to stress or to differ with. In the first place with regard to a specific gravity of 1010–1030, undoubtedly this is within the normal range, but for our purposes all cases which have had albumin or casts are suspicious cases, and must do better than the normal range. We insist upon a specific gravity of at least 1020 in any case that has had albumin, or albumin and casts, and we consider that to be of very great importance.

The value of any statistics or an appreciation of their value

must depend, of course, on the class being homogeneous and a knowledge of how the units were selected. We did not contribute to the work of the Special Committee, not for the same reason that Dr. Symonds did not contribute—that we had no such cases—but for a different reason, because we had been accepting these cases on a single examination if it measured up to our standards and we had practically no material of which we could say that it was an intermittent albuminuria. I do not like the name.

With regard to nucleo-proteid we have not gone into that very much. We have doubted the practical ability of our men to distinguish and, in our own office or when examined by our own chemist, we have assumed that anything that reacted to Heller's Test in a typical way was serum albumin no matter how slight.

With regard to the amount of albumin or the number of casts, in the slighter traces within reasonable limits we do not find that there is any material difference or believe that there is any material difference as to whether it is the slightest possible trace, a slight trace, or a very slight trace. If it is a quarter or a half per cent., that is a different story, but this occurs rarely in our business, and I quite agree with Dr. Russell on that. But I want to say that I differ with him on this question of urea. I can see the older members smile as this is a point upon which I am supposed to be insane. It will continue to be a fixed idea so long as present conditions exist, so long as the 75% of our Association, who agree with Dr. Russell, get a very high mortality or a moderately high mortality with the cases selected on their basis, and so long as those companies who consider urea of some importance, or of very great importance, obtain a low mortality. We consider that the relation between urea and specific gravity is of the utmost importance, and if we have been fortunate enough to obtain a low mortality for fifteen years, I believe it is because we have included those two factors of urea and specific gravity as in our original test.

Dr. Archibald—I am very glad that Dr. Dwight has men-

tioned that point about urea, because this is something I want to speak of. At the last meeting of the British Medical Association, the question of renal efficiency was discussed by many of the authorities in England. Among others, Dr. Hugh McLean. In his paper he made a very comprehensive analysis of the data. The various tests for renal efficiency were dealt with, and the conclusion he reached, irrespective of the amount of albumin and casts in the urine, was that the simplest method was the "Concentration Urea Test," as follows:

The patient was given 15 grams of urea in 100 cc. of water. and the urine tested one hour and two hours after. If the urine showed 2% or more of urea in either specimen, but preferably in the specimen two hours after ingestion, the kidney function could be counted as normal.

This conclusion was reached by Dr. McLean after checking up a large number of cases tested by various other methods. The article interested me very much and the method is one which could, I believe, easily be applied in the case of applicants for insurance.

Dr. Symonds—I would like to ask Dr. Dwight how long he has been working with this one test?

Dr. Dwight—About seventeen years.

Dr. Symonds—With satisfactory results?

Dr. Dwight—Entirely so.

Dr. Symonds—Do you microscope the specimens?

Dr. Dwight—All of the doubtful cases.

Dr. Symonds—And you require 1.020?

Dr. Dwight—Yes, 1.020.

Dr. Symonds—I am glad to say that we are following in your footsteps, because that is the method we have adopted, plus not taking cases that are below what we call our best weight in the younger ages. We do not do much in the urea line, but in the young ages we require them to come up to much above the average weight for their age before we accept them.

Now with regard to nucleo-protein, we have kept a record of this for a good many years, and it is reported only by our Home Office examiners and by our medical officers, so that the results

are reasonably reliable. You see they are poor, nothing like as poor as the albuminurics which show 135, while the nucleo-proteins show 105,—in other words they stand about midway between the normal mortality of the Company and the albuminurics; but I do not feel satisfied yet with that class,—it is a class that will bear a little further investigation. It is of importance, Dr. Exton to the contrary, and nucleo-protein if it is present should not be wholly ignored, or passed over absolutely, and any preparation which contains acetic acid is very apt to give a reaction of nucleo-protein in the urine. I have tested a great many of them. I started to test them twenty-five years ago, and that was the one stumbling block I found, that where acetic acid was used, one is apt to get a reaction in a certain number of cases for nucleo-protein, and for that reason I think it would be a mistake to use that particular preparation as a qualitative test for albumin.

The sulpho-salicylic acid we have used in our office with very satisfactory results. It gives excellent results promptly and quickly, and I am coming to the opinion that it is as good and as reliable, perhaps, as Heller's, and of course a good deal prompter. In order to get satisfactory results with Heller's Test you must let it stand for fifteen or twenty minutes, but with the salicylic acid you get a reaction in a minute by the contact method.

Dr. Dwight—When we get a report of nucleo-protein we treat the case as though albumin had been reported, and we make that case respond to all the tests which we would require if it had been serum albumin. In some of the large cities, for example in Philadelphia, we have a great deal of difficulty in getting our examiners to use anything but heat and acetic acid, and they are reporting albumin all the time, so much so that we have instructed our Chief Examiner that whenever he finds albumin by any test he is to send it to the chemist. We treat the case as albumin until we get a report from the chemist and it is only upon his statement that we decide we will take the case.

Dr. Knight—The men who specialize in the care of the tuber-

culous have learned to expect that whenever the process lights up and becomes active in the patient, they are going to get albuminuria, and in that same patient when the process is quiet they do not get it. I know that Dr. Dublin was up at a conference in our sanatorium, and I will ask him to tell us whether that point was brought up.

Dr. Dublin—Yes, that particular point was referred to, and they all recalled exactly what Dr. Knight says, that when these cases of tuberculosis would become active and light up, they found albumin. I am inclined to think that the presence of albumin in these young people is not an indication of a permanently disturbed kidney but rather a subtle indication of the existence of tuberculosis, and that you are dealing with people who are apt to break down with tuberculosis and this is one of nature's ways of showing it. That was exactly the thought, as I remember it, in the minds of those experts in tuberculosis.

Dr. Knight—Generally at the sanataria they keep people for about six months. We keep them all the way from six months to six or seven years, and we follow our cases with more care than they are generally followed, and we find that, as the process is arrested and stays arrested, the albuminuria disappears, and as they have their exacerbations, back comes the albumin.

Dr. Russell—Mr. President, the point that you and Dr. Dublin brought out is the point that we wished to emphasize in this paper, and is one of the main things that we wanted to call to the attention of the Association. In regard to what Dr. Archibald says about the urea test, if he relies entirely upon that in his albuminurics and his cases showing casts, I believe there will be bad results in those cases, for the reason that practically all authorities are agreed that you can get a normal function test in the kidney and have a destruction of a considerable portion of the kidney.

Dr. Knight—I am sure that we all thank Dr. Russell for his very interesting and most excellent paper.

The President announced the appointment of the following Committees:

Committee to study Dreyer's work in relation to life insurance, as set forth in Dr. Dublin's paper:

DR. O. H. ROGERS, *Chairman*,
DR. T. H. ROCKWELL,
DR. BRANDRETH SYMONDS,
DR. WILLIAM MUHLBERG,
DR. J. ALLEN PATTON,
DR. LOUIS I. DUBLIN.

Committee to study the question of the standardization of medical blanks:

DR. BRANDRETH SYMONDS, *Chairman*,
DR. HARRY TOULMIN,
DR. HENRY WIREMAN COOK.

On motion by Dr. Hobbs, seconded by Dr. Symonds, the following resolution was adopted:

RESOLVED—That the members of the Association of Life Insurance Medical Directors extend a vote of thanks to the officers of the Metropolitan Life Insurance Company, for their kindly and hospitable treatment as guests of the Company, during the meeting now closing, and that the Secretary convey to them in writing this expression of appreciation.

Dr. Knight—I am sure I know how you all feel. Your patience and your generosity to the chair during this meeting has been most delightful. I cannot imagine anything that would make a man more happy, and I have every right to say to Dr. McMahon

that instead of having hardship and care in the year ahead of him, he is going to have a most delightful time, as I have had. If it has been as pleasant for the members as it has been for me, the meeting has been most successful.

Dr. Symonds—I have one more motion to make. I move a vote of thanks to our retiring President for one of the best meetings we have ever had. Dr. Hobbs seconded the motion and it was carried by a rising vote.

On motion the meeting adjourned sine die.

The Annual Dinner of the Association of Life Insurance Medical Directors was held on the evening of Thursday, October 20, 1921, at the Metropolitan Life Insurance Company. The following members were present:

John L. Adams, H. B. Anderson, A. W. Balch, J. T. J. Battle, W. W. Beckett, D. N. Blakely, C. T. Brown, C. R. Burr, F. W. Chapin, L. D. Chapin, C. L. Christiernin, Henry Colt, J. N. Coolidge, T. C. Craig, R. M. Daley, E. J. Dewees, E. G. Dewis, W. W. Dinsmore, P. G. Drake, L. I. Dublin, E. W. Dwight, O. M. Eakins, W. G. Exton, E. L. Fiske, Paul FitzGerald, L. K. Frankel, R. A. Fraser, S. W. Gadd, Homer Gage, W. S. Gardner, Arthur Geiringer, A. H. Griswold, F. L. Grosvenor, G. C. Hall, W. J. Hammer, G. A. Harlow, Frank Harnden, A. B. Hobbs, E. M. Holden, Arthur Hunter, Ross Huston, W. G. Hutchinson, C. B. Irwin, W. A. Jaquith, A. O. Jimenis, A. E. Johann, G. E. Kanouse, R. J. Kissock, A. S. Knight, W. W. Knight, R. L. Lounsberry, T.

F. McMahon, H. A. Martelle, S. W. Means, J. C. Medd, William Muhlberg, Herbert Old, M. I. Olsen, J. A. Patton, W. A. Peterson, C. B. Piper, J. E. Pollard, J. T. Priestley, F. P. Righter, T. H. Rockwell, O. H. Rogers, M. J. Rosenau, R. L. Rowley, E. F. Russell, S. B. Scholz, Jr., J. M. Smith, Morton Snow, H. B. Speer, J. B. Steel, Harry Toulmin, J. P. Turner, H. G. Tuttle, F. L. Truitt, C. A. Vandervoort, W. R. Ward, W. H. E. Wehner, F. S. Weisse, E. A. Wells, F. L. Wells, C. F. S. Whitney, T. H. Willard, M. C. Wilson, Glenn Wood.

LIST OF MEMBERS OF THE ASSOCIATION OF LIFE INSURANCE MEDICAL DIRECTORS

Frederick R. Abbe.	Columbian National, Boston, Mass.
John L. Adams, M.D.	Metropolitan, New York, N. Y.
Edwin H. Allen, M.D.	John Hancock, Boston, Mass.
Charles D. Alton, M.D.	Connecticut Mutual, Hartford, Conn.
John W. Amesse, M.D.	Capitol Life of Colorado, Denver, Colo.
Harry B. Anderson, M.D.	Imperial Life, Toronto, Ont.
Thomas D. Archibald, M.D.	North American, Toronto, Ont.
William Armstrong, M.D.	Connecticut General, Hartford, Conn.
Alfred W. Balch, M.D.	Metropolitan, New York, N. Y.
William B. Bartlett, M.D.	John Hancock, Boston, Mass.
John T. J. Battle, M.D.	Jefferson Standard, Greensboro, N. C.
Wesley W. Beckett, M.D.	Pacific Mutual, Los Angeles, Calif.
Charles D. Bennett, M.D.	Mutual Benefit, Newark, N. J.
Thomas W. Bickerton, M.D.	New York Life, New York, N. Y.

Edward B. Bigelow, M.D.	State Mutual, Worcester, Mass.
Albert W. Billing, M.D.	Equitable, New York, N. Y.
Cecil C. Birchard, M.D.	Sun Life, Montreal, Quebec.
Arthur B. Bisbee, M.D.	National, Montpelier, Vt.
David N. Blakely, M.D.	New England Mutual, Boston, Mass.
Robert J. Blanchard, M.D.	Great West, Winnipeg, Man.
William M. Bradshaw, M.D.	Mutual, New York, N. Y.
Frederick G. Brathwaite, M.D.	Equitable, New York, N. Y.
Leverett D. Bristol, M.D.	Penn Mutual, Philadelphia, Pa.
Chester T. Brown, M.D.	Prudential, Newark, N. J.
William H. Browne, M.D.	American Life, Detroit, Mich.
Chauncey R. Burr, M.D.	Metropolitan Life, New York, N. Y.
Joseph T. Cabaniss, M.D.	Travelers, Hartford, Conn.
Frank H. Carber, M.D.	Mutual, New York, N. Y.
Frank W. Chapin, M.D.	Home Life, New York, N. Y.
Laurence D. Chapin, M.D.	Mass. Mutual, Springfield, Mass.
John P. Chapman, M.D.	Penn Mutual, Philadelphia, Pa.
Charles L. Christiernin, M.D.	Metropolitan, New York, N. Y.
Charles P. Clark, M.D.	Mutual Benefit, Newark, N. J.
Henry Colt, M.D.	Berkshire, Pittsfield, Mass.
Edwin A. Colton, M.D.	National, Montpelier, Vt.
Henry W. Cook, M.D.	Northwestern Nat., Minneapolis, Minn.
John N. Coolidge, M.D.	Metropolitan, New York, N. Y.
Robert M. Daley, M.D.	Equitable, New York, N. Y.
Edwin Grafing Dewis, M.D.	Prudential, Newark, N. J.

Wilton E. Dickerman, M.D.	Ætna, Hartford, Conn.
Henry K. Dillard, M.D.	Penn Mutual, Philadelphia, Pa.
William W. Dinsmore, M.D.	Travelers, Hartford, Conn.
Percy G. Drake, M.D.	Travelers, Hartford, Conn.
Edwin W. Dwight, M.D.	New England Mutual, Boston, Mass.
O. M. Eakins, M.D.	Reliance, Pittsburgh, Pa.
Z. Taylor Emery, M.D.	Manhattan, New York, N.Y.
Calvin H. English, M.D.	Lincoln National, Fort Wayne, Ind.
William G. Exton, M.D.	Prudential, Newark, N. J.
John Ferguson, M.D.	Excelsior, Toronto, Ont.
John W. Fisher, M.D.	Northwestern Mutual, Milwaukee, Wis.
Paul Fitzgerald, M.D.	Prudential, Newark, N. J.
Robert A. Fraser, M.D.	New York Life, New York, N. Y.
Samuel W. Gadd, M.D.	Philadelphia Life, Philadelphia, Pa.
Homer Gage, M.D.	State Mutual, Worcester, Mass.
William S. Gardner, M.D.	Mutual Life, New York, N. Y.
Donald M. Gedge, M.D.	Metropolitan, New York, N. Y.
Arthur Geiringer, M.D.	Equitable, New York, N. Y.
Alvah H. Gordon, M.D.	London and Scottish, Montreal, Que.
Angus Graham, M.D.	London Life, London, Ont.
Fred Le M. Grasett, M.D.	Canada Life, Toronto, Ont.
Arthur H. Griswold, M.D.	Phœnix Mutual, Hartford, Conn.
Frank L. Grosvenor, M.D.	Travelers, Hartford, Conn.
Ignatius Haines, M.D.	John Hancock, Boston, Mass.
George C. Hall, M.D.	Life Ins. Co. of Virginia, Richmond, Va.

Joseph B. Hall, M.D.	Connecticut Mutual, Hartford, Conn.
William F. Hamilton, M.D.	Sun Life, Montreal, Que.
William J. Hammer, M.D.	New York Life, New York, N. Y.
George A. Harlow, M.D.	Northwestern Mutual, Milwaukee, Wis.
Frank H. Harnden, M.D.	Travelers, Hartford, Conn.
Calvin L. Harrison, M.D.	New York Life, New York, N. Y.
Ernest M. Henderson, M.D.	Confederation Life, Toronto, Ont.
John U. Hobach, M.D.	Penn Mutual, Philadelphia, Pa.
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